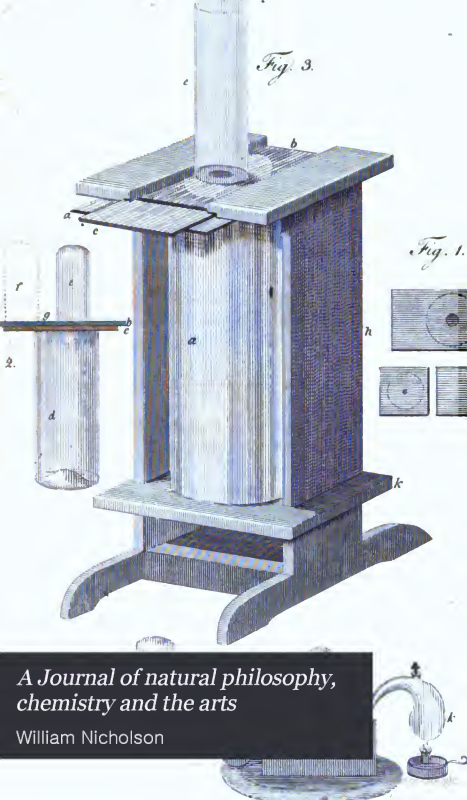

A Journal of natural philosophy, chemistry and the arts

William Nicholson

A

JOURNAL

OF

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CHEMISTRY,

AND

THE ARTS.

VOL. XV.

Illustrated with Engravings.

BY WILLIAM NICHOLSON.

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PREFACE.

THE Authors of Original Papers in the present Vo- R. L. Edgworth, Esq.; J. G.; Mr. J. Sadler; J. Horsburgh, Esq.; R. B.; Mr. Charles Sylvester; Jonathan Stokes, M. D.; Rev. Johnson Grant, A.B; Mr. J. Bennett; J. E. Conant, Esq.; Dr. Oersted; Mr. William Skrimshire, Jun.; S Taylor, Esq.; A Correspondent.

Of Foreign Works, Professor Ritter; Messrs. Desormes and Clement; M. Vauquelin; A. Laugier; Professor Proust; Klaproth; Robiquet; Dispan.

And of English Memoirs abridged or extracted, C. Hatchett, Esq. F. R. S.; W. H. Wollaston, M. D. Sec. R. S.; Count Rumford, F.R.S.; Mr. W. Wallis Mason; Mr. Gilbert Gilpin; Mr. Thomas Parker; J. C. Curwen, Esq. M. P.; A. Carlisle, Esq. F. R. S. F.L.S.; S. A. Bardsley, M. D.; W. Herschell, LL. D. F.R.S.; Mr. John Mayow; Mr. W. Smith.

Of the Engravings the Subjects are, 1. The Furnaces and Apparatus at present used for refining Lead. By John Sadler. 2. An Odometer, for measuring the Distance run by a Carriage. By R. L. Edgworth, Esq. 3. 4. Figures to illustrate the Propagation of Electricity. By Dr. Oersted. 5. Diagram for describing the Wheels and Pallets of Graham's Escapement for Clocks. By Mr. Bennett. 6. An Escapement of Thiout. 7. Machine for facilitating the Work of Shoemakers. 8. An improved Crane. By Mr. Gilbert. 9. Method of using the common Chain in Pullies to greater Advantage than Ropes. 10. A Drawing, representing the Structure and explaining the Motion of Fishes. By A. Carlisle, Esq. F. R. S. 11. 12. Diagrams to exhibit the proper Motion of the Sun. By W. Herschell, LL. D. F.R.S. 13. A new portable Blow-pipe. By W. H. Wollaston, M. D. Sec. R. S. 14. Plan of Six Acres of Water Meadow, as improved by Mr. William Smith. 15. Improvement in the Striking Part of Clocks. By Mr. Ward. 16. A Machine for cutting the Edges of Books. By Mr. Hawkins. 17. Machine for the transfer of Boats through the Locks of Canals with the least possible consumption of Water.

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SUPPLEMENT.

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SEPTEMBER, 1806.

ARTICLE I.

The Process for refining Lead, as practised in England. In a Letter from Mr. JOHN SADLER.

MY DEAR SIR,

CITZ. DUHAMEL, in his Memoir on the Refining of Lead in the large way, has given a sketch of the process used in England; if you think the following more detailed description will be acceptable to the readers of the Philosophical Journal, it is at your service. Duhamel on lead works.

The object of refining lead is not merely on account of the silver it contains, but to procure it as free as possible from the other metals with which it is usually alloyed, and to procure litharge. The silver is only an object so far as it helps to pay the expense of refining. Object of refining lead.

The lead produced at the smelting hearths or furnaces in England is never perfectly pure; it is always alloyed with a portion of silver, and most commonly with one or most of the following metals; viz. zinc, antimony, copper, and arsenic; which render it unfit for some of the purposes to which lead is applied. Usual impurities of English lead besides silver.

It is refined by vitrification and reduction.

The operation of refining is founded on the facility with which lead is oxidated when exposed to heat in contact with atmospheric air, and the peculiar properties the oxides of lead possess; being easily fused, and in that state oxidating and combining with most of the metals; gold, silver, and platina excepted.

upon a cupel in a reverberatory furnace.

The lead to be refined is exposed to the action of heat and air upon a *cupel* or *test*, composed of a mixture of bone and fern ashes in a reverbatory furnace; the description of which, with the different manipulations, are as follows:

Description of the furnace for refining lead.

The refining furnace is composed of good solid masonry, bound together with iron bolts. It differs very little in its construction from the common reverbatory furnace, except the bottom, which is perforated to receive the test or cupel.

Fig. 1. Plate I. is a perspective view of the furnace with its iron work; *a* the teazing hole, *b* aperture by which the test is supplied with lead, *c* an arch or dome over the feeding hole, communicating with the furnace stack by a flue, *d* area or space where the test is taken in and out the furnace, *e e* two strong iron bars to support the test when in its place, *f* cast-iron pot set in masonry, the flue passing into the stack of the furnace, *g* the stack, *p* the ash pit, *q* an iron bar to slide the ladle on when feeding the test.

Fig. 2. a perpendicular section of the furnace shewing the test *i*, supported in its place under the opening of the bottom of the furnace by the two wedges *r r*; *k* aperture for the nozzle of the bellows, *s* fire bar resting on the bearers.

Fig. 3. plan of the interior of the furnace; *l* part of the bellows, *h h* flues from the body of the furnace to the stack.

The same letters in the different plans are meant to denote the same parts.

Plate II. Fig. 1. plan of the iron frame into which the mixture of bone and fern ashes is rammed to form the test. This frame is something larger than the elliptical hole in the bottom of the furnace.

Figs. 2 and 3. plan and section of the test; *m* the part which

which contains the lead to be refined, *n* breast of the test, *o o* small gutters or channels through which the litharge flows, *p* a semi-eliptical hole for the litharge to fall throngh from the gutters upon the area of the refinery.

These drawings and references will be sufficient to make the description of the furnace, &c. clearly understood.

Of the Test or Cupel.

A good test is of the first importance in refining; the method of constructing one I shall endeavour to point out. Six parts of well bnrnt bone ashes and 1 part of good fern ashes are to be well mixed, sifted through a sieve (the spaces in which are about $\frac{1}{8}$ of an inch square), and moistened to about the same degree the founders use their sand. The iron frame is to be laid on the floor and made steady, with wedges under its rim; about two inches in thickness of the ashes are to be equally spread over the bottom, and with an iron beater, such as used by the founders, equally rammed between the cross bars; the frame is to be again filled and rammed all over, beginning at the circumference and working spiral ways until finished in the centre, the filling and ramming to be repeated until the frame is completely full; an excavation to contain the lead is made as expressed in the plan, with a sharp spade about 5 inches square, the edges dressed with a long-bladed knife; a semi-eliptical hole, as at *p*, is to be cut through the breast. Having proceeded so far, the test is to be turned on its side and dressed from all superfluous ashes adhering to the bottom, taking care that none shall be left flush with the bottom of the frame or cross bars, otherwise in fixing the test to its situation at the bottom of the furnace it would be liable to be bulged.

The test is made of 6 parts bone and 1 fern ashes.

Fixing the Test in its situation.

The rim of the test is now to be plastered with clay or moistened ashes, placed upon the supporting cross bars, and fixed with wedges firmly against the bottom of the furnace, the breast next to the feeding hole.

Manner of fixing the test.

A gentle fire may now be lighted, and gradually increased until the test be red hot. When it ceases to emit steam from the under side it is sufficiently dry.

Lead previously melted in the iron pot *f* is ladled into the

Manipulation by which the

litharge is removed from the charge of fused lead, put into the test, &c.

the test until the hollow part be nearly filled, the operator closes the feeding aperture, and increases the heat of the furnace until the surface of the lead is well covered with litharge; he then removes the door from the feeding hole, and with an iron rod, which has one end bent down at right angles about 3 inches and made flat or chissel-shaped, scrapes the small gutter or channel *o* until the litharge just flows into it, the blast from a pair of double bellows is then directed from the back part over the surface of the test, the litharge is urged forward, and flows from the gutter upon the floor of the refinery; the operation now goes forward, gradually adding lead as the escape of litharge makes necessary, until the gutter is so worn down that the test does not contain more than an inch in depth of lead, the blast is then taken off, the gutter filled up with moistened ashes, and a fresh one made on the other side the breast; the test is again filled, though not so full as at first, and the operation carried on until this gutter also is worn down and the test contain from about 50 to 70 lbs. of alloy. This quantity is run into an iron pot, and set by until a sufficient number of pieces have been collected to make it worth while to take off a plate of pure silver from them.

Alloy left behind.

The quantity of alloy left in the working off each test must depend in a great measure upon the quantity of silver it by estimation is supposed to contain. A sufficient quantity of lead should always be left in the alloy to make it fuse easily in the iron pot.

Some litharge soaks into the test.

When the test is removed from the furnace and broken up, the litharge will be found to have penetrated to an inconsiderable but equal depth in the ashes; that part not impregnated with litharge may be pulverised, mixed with fresh ashes, and again used for another test.

The silver is refined by oxiding the lead it still contains.

The operation of taking off the silver pure differs in no respect from the foregoing, only more care is observed in the working, not to suffer the escape of any metallic particles with the litharge, as that would occasion considerable waste of silver. As the process advances, and the proportion of silver to lead increases, the litharge assumes a darker colour, a greater heat becomes necessary, and at last the brightening takes place; the interior

Smelting of Lead

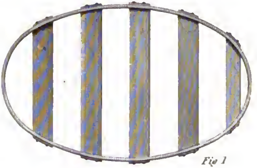

Fig 1

Fig 2

Fig 3

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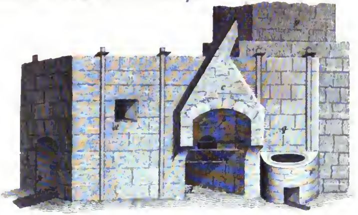


Smelting of Lead

Fig 2nd

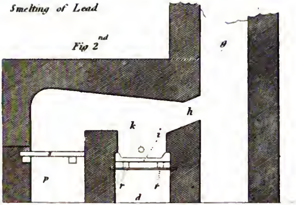
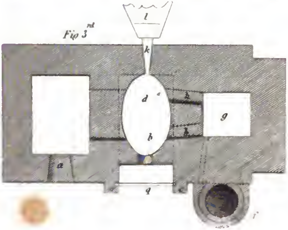


Fig 3rd



of the furnace, which during the whole of the process had been very obscure and misty, clears up. When the operator observes the surface of the silver to be free from litharge, he removes the blast of the bellows, and suffers the furnace to cool gradually; as the silver cools many protuberances arise on the surface, and fluid silver is ejected from them with considerable force, which falling again on the plate spots it very fantastically with small globules.

The brightening.

Silver ejected from the mass as it cools.

The latter portions of litharge bring over a considerable quantity of silver with them; this is generally reduced by itself and again refined.

The last portions of litharge contain silver.

The litharge as it falls upon the floor of the refinery is occasionally removed; it is in clots at first, but after a short time as it cools it falls for the most part like slacked lime, and appears in the brilliant scales it is met with in commerce: if it is intended as an article for sale, nothing more is necessary than to sift it from the clots which have not fallen and pack it in barrels.

The litharge flakes in cooling.

If, on the contrary, it is intended to be manufactured into pure lead, it is placed in a reverbatory furnace, mixed with clean small-coal, and exposed to a heat just sufficient to fuse the litharge. The metal as it is reduced flows through an aperture into an iron pot, and is cast into pigs for sale. During the reducing, care is taken to keep the whole surface of the litharge in the furnace covered with small-coal.

It is reduced by keeping it covered with small-coal at a moderate heat.

In some smelt works, instead of a reverbatory furnace for reducing, a blast furnace is made ufe of, on account of the greater produce, but the lead so reduced is never so pure as that made in the wind furnace. The oxides of the metals, which require a greater heat to reduce than the lead, are in the blast furnace generally reduced with it.

The reduction is best performed in the wind furnace.

The volatile oxides, as zinc, antimony, and arsenic, are mostly carried off by evaporation during refining; a considerable portion of the oxide of lead itself is carried off by evaporation, making the interior of the furnace so misty and obscure that a person unused to refining cannot see more than a few inches into it.

Volatile oxides

A considerable portion of these oxides are driven by the

carried up the chimney.

the blast of the bellows through the feeding aperture, and would be dissipated in the refining-house, to the great injury of the workmen's healths; to prevent their ill effects the arch or dome over the feeding hole is erected to carry the fume into the stack of the furnace.

II.

Facts and Observations relating to the Winds, Waves, and other Phenomena by which the Surface of the Sea is affected. In a Letter from James Horsburgh, Esq.

To Mr. NICHOLSON.

Sir,

On the surface of the sea.

FROM reading in your Journal Vol. 12th. the account of the Sciches of the Lake of Geneva, I have been induced to send to you a few remarks relative to the surface of the sea, which are more particularly applicable to the Indian and Chinese Seas, where these observations were made.—

The smooth regular waves of a steady breeze are augumented by the rise of a low cloud though the breeze is diminished.

When a steady breeze of wind has continued to blow for any length of time, with a clear sky, or small clouds high in the atmosphere, the waves are generally regular and smooth, gliding in the direction of the wind; particularly when there is no current :—At such times, if a dense cloud is generated, and is low in the atmosphere when passing over the observer, the strength of the regular breeze is decreased, and the waves appear to be agitated by the cloud whilst it passes over them; their summits being more elevated and turbulent :—but no sooner has the dense cloud passed the zenith of the observer, than the breeze resumes its former strength, and the waves glide along smooth as before.

It seems as if those clouds had a directaction on the surface.

When many of these dense clouds are generated, and follow the course of the prevailing wind, in succession, the waves become turbulent and irregular, particularly when these clouds are near the surface of the sea, accompanied by showers of rain: this has frequently been observed in the Indian Seas, and often inclined me to think these low dense clouds had some affinity with the surface of the sea.

The

The effects proceeding from these dense clouds during their passage over the zenith, are the opposite to those experienced from a regular squall :—for this generally first makes its appearance by a small arched cloud, either rising from the horizon, or formed at a small distance above it ; which rises gradually until near the zenith. These effects are opposite to those of a squall

When the preceding clouds of the arch approach the zenith, the strength of the squall commences, and continues strong during the passage of the clouds over it ; which is the reverse of what has been noticed as the result proceeding from dense clouds generated at high altitudes.—Currents or ripplings on the surface of the sea, seem to have an affinity with the wind.—Where tides run strong, in the entrance of great rivers and other places, the strength of the wind is often observed to be modified by the tide ; blowing strong on the flood, and moderate on the ebb, when the direction of the wind is into the rivers, or nearly in the line of the flood tide. Effects of the squall. Currents and tides affect the wind.

The following peculiarity of a sudden variation in the strength of the wind, has frequently been observed in low latitudes.—In settled weather, when a brisk and regular wind is experienced in deep water, if there are any shoal banks or coral flats of considerable extent, the strength of the wind is often perceived to be much less on these banks or flats, than in the deep water ; more particularly if eddies or ripplings, occasioned by tide or currents prevail on the banks at the time.—I have often observed in g tting on the edge of one of such banks, that the strength of the regular breeze instantly abated ; but resumed its regular force on departing from the verge of the bank into deep water :—this has been experienced repeatedly ; and I have found a ship hardly manageable from a deficiency of wind on the verge of a bank with shoal water on it ; when at a small distance from it in deep water, some of the light sails were obliged to be taken in, the regular breeze over the surface of the deep water, prevailing so strong. Upon banks or flats the wind is suddenly and considerably less than on the deep water.

In several parts of the Indian Seas, particularly to the eastward of the Nicobar Islands, between Achen Head and Junkseylon, very strong ripplings prevail during the south-west monsoon.—When these ripplings are very high and numerous, there is seldom any current experienced ; Very strong ripplings in the Indian Sea, with temporary diminution of wind.

ced ; which appears singular, for they are (I believe) generally supposed to be the effect of currents.

These ripplings extend in long narrow ridges, with smooth spaces between them of considerable extent:—they are alarming to strangers in the night, from the noise occasioned by the broken water.—The collision of the water in these ridges, produces breakers so high, that at times it would be dangerous to risk a boat amongst them, although the weather was serene.—They move with considerable velocity: when they pass a ship, a decrease of the strength of the wind accompanies them; a trembling motion is given to the vessel by the great collision of the broken water, and frequently some spray is thrown on the deck.—The ridges are seldom more than a few minutes in passing ; the wind then resumes its former strength, which continues regular until another ridge assails the ship.—Probably these must proceed from the south-west monsoon blowing from the ocean, round Achen Head, into the entrance of Malacca Straits ; but it is singular that no currents are experienced with these high ripplings.

Phenomena of currents.

In the ocean, and also in narrow seas, currents frequently raise the sea, and agitate the surface greatly.—When the wind and current coincide in their direction, the sea generally is moderately smooth ; but it is agitated, and turbulent waves are produced, when the current runs in a contrary direction to the wind.—This is a general remark among nautical men, which often holds good ; although it does not always ensue : for turbulent waves are sometimes the effect of a strong current, when the direction of it and the wind agree.—It is singular that currents are very changeable in some parts of the ocean, far distant from land ; particularly near the equator.—I have several times, in low latitudes, experienced the current run upwards of sixty miles in twenty-four hours, to the eastward or westward ; then change suddenly, and set with equal velocity in the opposite direction, during the subsequent twenty-four hours.—The rise and fall of tides in most parts of the globe, appear to be much greater in high latitudes, than within the tropics : though currents seem to prevail more here, than in situations of the former

—very changeable.

mer kind.—In the Northern Atlantic they are seldom strong: but are frequently so near the equator, between the Coast of Guinea and the American Continent. About the southern limit of the Maldiva Isles, near the Equator, and to the eastward of the Philippine Islands, they are frequently strong and changeable.—In latitude 40°. South near the Cape of Good Hope, a strong current commenced suddenly, which produced a mountainous sea, when there was very little wind:—it continued to run strong for a day, then suddenly abated, and set in another direction with a gentle velocity; the high sea falling at the time.

Smooth and agitated portions of the sea with faint airs.

The agitated and smooth portions observed on the lakes, prevail much at sea in sultry weather, when nearly calm. At such times, the faint airs seldom agitate the surface of the sea in a regular manner, but the agitated and smooth portions, appear in veins and patches, intersecting each other in a variety of directions.—These appearances continue for days together, when faint airs and calms are experienced between the tropics: the faint airs are generally irregularly felt; sometimes gentle; at other times very weak, inclining to calm.—The surface of the sea to a considerable distance around a ship, always appears more smooth at these times, than at greater distances, towards the horizon; which often is the cause of belief in an approaching breeze, never realized.

The perspective makes the distant sea appear roughest.

Small medusæ and insects out at sea.

In low latitudes, when calms and faint airs have been experienced for two or three days; or for a longer period, I have frequently perceived the surface of the ocean have an oily appearance, with minute medusæ floating on it in great quantities.—They seemed to be interspersed over the smooth and agitated portions, and not confined to the smooth places.—Small insects, some with, and others without wings, have often been seen skipping on the surface of the sea in calm weather, many degrees distant from land.

Smooth places are seen during showers.

The smooth veins on the surface of the sea are also concomitant with rain; particularly at the commencement of showers, when there are gentle breezes of wind:—and sometimes appear to indicate rain.

Other facts respecting them.

Smooth veins on the surface of the sea prevail to the westward of the Laccadiva Islands, between these and the

 Island

Island Socotra, in the months of March and April; and are most perfectly depicted during brisk winds.

In these months the winds blow from the northward, in moderate and strong breezes, at a few degrees distance from the Coasts of Canara and Concan; and are mostly from N. N. W. to N. by E.—these winds do not blow uniformly, although the sky is generally clear, but come in gusts at short intervals; particularly in the night, the breezes being then stronger than in the day.

It is very common with these winds, to observe smooth veins on the surface of the sea, which extend in lines parallel to each other, and to the direction of the wind: they are often discernable in the night, when the moon exhibits no light, being so different in colour from the other parts these having a black appearance, occasioned by the fresh (or brisk) breezes agitating the surface and producing a great contrast between these agitated portions and the smooth veins.

Curious fact of a white dust deposited at sea.

Another curious phenomenon has frequently been observed to accompany these northerly winds, which is; in March or April, ships that are bound to Bombay or Surat, frequently have their rigging covered with white dust, although several degrees distant from the coast of Canara or Concan. The northerly and north north west winds, blowing from the coast of Persia, over an extensive surface of sea, (at least ten or twelve degrees) it is difficult to judge what can occasion the dust, if it is not generated in the atmosphere, which is in these months sometimes impregnated with a dry haze.

The gulph weed in the Atlantic Ocean is deposited in long parallel veins in the direction of the wind.

It may be observed, that similar to the smooth veins here mentioned, lying in the direction of the wind, is the direction of the veins or strata of Gulph Weed, in the middle of the Atlantic Ocean. The southern limit of this marine vegetating substance is about 22°. or 22½° north latitude, or near the tropic of Cancer; and the northern limit seems to be about the 42° of north latitude. It is always seen in long veins, or strata, parallel to each other, in the direction of the wind. When the wind changes, the veins of weed appear to be disturbed for a time; they are however not long before their direction is in conformity with the wind. These veins of weed are governed in their direction

direction by the wind, whether the sea be smooth, or high; and in general do not appear to be more than from twelve to twenty hours, in changing their direction.

In No. 57 of your Journal, in a reply to M. M. your correspondent, the squall is thought to be occasioned by a descending wind, produced by the impulse of falling rain. This suggestion seems to agree with your observation, for I have several times, in calm weather, seen a cloud generate and diffuse a breeze on the surface of the sea, which spread in different directions from the place of descent. A remarkable instance of this occurred in Malacca Strait during a calm day, when a fleet was in company: a breeze commenced suddenly from a dense cloud, its centre of action seemed to be in the middle of the fleet, which was much scattered. This breeze spread in every direction from a centre, and produced a singular appearance in the fleet, for every ship hauled close to the wind as the breeze reached her, and when it became general, exhibited to view the different ships sailing completely round a circle, although all hauled close to the wind.

The author agrees with W. N. that the squall is a descending wind.

Remarkable instance in proof.

With this descending wind there was no rain fell on the ships at the extremities of the fleet, but a partial shower was observed to have fallen on the ships in the centre. Notwithstanding what has been just observed, squalls or brisk winds which commence suddenly after calms, are generally experienced to have their motion in a horizontal direction, when the impulse is perceived on the sails of a ship; but it appears probable, that the current of wind may descend until near the surface of the sea, when calm over the surface; and then be deflected in a horizontal direction on approaching it.

Your correspondent, M. M. thinks the velocity of the swell of the sea not greater than eight or ten miles an hour. It is much greater in general, but mutable according to circumstances.

The velocity of the swell is greater than supposed by M. M.

The velocity of the swell (or waves) in a strong breeze or trade wind, probably is about twenty miles an hour; for they pass a ship fast when she has a velocity of ten or eleven miles an hour in the same direction as the waves. At such times the velocity of the waves is easily measured with the common log, by noticing with a pro-

How to measure, &c.

per quantity of line out, when the log is lifted on the top of a wave, marking this time, and measuring the interval from it to the time the ship's stern is lifted up by the same wave, by a watch with a second hand. The length of the line from the stern, compared with the interval of time, will give the excess of the velocity of the wave over that of the ship, and these added together will be the velocity of the wave. The velocity of the swell may also be obtained when calm, by sending a boat in the direction of the swell, to a moderate distance from the ship, having a line fast to each, to enable their distance to be measured. With a watch, measure the interval to the nearest second, when the boat was lifted up, by a swell, to the ship being lifted by it; compare the interval of time with the distance measured by the line to obtain the velocity of the swell. Several observations may be made in either case, and the mean taken as the result.

Less in shallow water.

The velocity of the waves seems generally less in shallow water, than in the ocean; the cause of this may be the resistance the particles of water meet with from mud or sand mixed in the water, or from friction against the ground.

Various swells at same time.

In the ocean it frequently happens that two swells run in directions opposing each other; at other times they cross each other obliquely: and sometimes three swells running in different directions, meet and run through each other; and continue to do so for a day or longer time, each retaining its own direction and apparent regular velocity.

The waves in the ty-fong.

It frequently happens during a ty-fong in the China sea, that the waves run in every direction; having the appearance of elevated mounts or pyramids, which infringe on each other with great violence. Ships are very liable to lose their rudders, when these pyramids strike against them; and the masts are endangered by the quick turbulent motion proceeding from such heterogeneous impulse.

Causes why the swell of the sea may precede the wind that caused it.

You remark that the swell caused by a storm, may be propagated with a greater mean velocity than the storm that causes it, and may therefore arrive on a coast before it, or come after the storm has ceased—this conclusion

seems just. The waves, (or swell) may be generated by a strong wind which has to contend with another blowing in opposition to it; this is frequently observed at sea; (seamen call it two winds fighting against each other) when this is the case, the velocity of a strong wind is greatly retarded, and its progress very slow whilst opposed by a breeze, although the latter be much inferior in strength:—and it frequently happens that a gentle breeze is prevalent over a strong wind, when the supply of the latter does not continue strong a sufficient length of time. The limit where two winds oppose each other, is sometimes observed to alter its position very little in two or three hours; a ship may continue to have a strong wind on one side of this limit for a considerable time, whilst a ship on the other side experiences a steady breeze from the opposite direction. It will therefore be easily comprehended, that when a strong wind has to overcome another wind, blowing in opposition to it, the velocity of the former must be slow until the latter is subdued, although the waves may speedily be agitated, and receive an impulse from it, by which they may greatly precede the wind that caused their formation. On the contrary, when a strong wind is diffused from the atmosphere, having no other wind to oppose it, the velocity of such must be greater than that of the waves formed by it; and consequently will precede the waves.

Opposing winds.

In September 1802, there was a storm on the south coast of China, in which a Spanish frigate and the Nautilus of Calcutta were lost. We were about five degrees from the coast at the time; had pleasant weather and little wind:—A high swell reached us, by which we were (I may say) certain that a storm had happened on the coast; and on our arrival in a few days afterwards, found it had been so.

Instance.

In December 1803, at anchor on the eastern sea-reef, at the entrance of Hooghley river, a gale of wind commenced, and blew from the northward, off the land: at the same time a heavy squall came rolling in from the sea, directly in opposition to the prevailing wind; this caused an apprehension that the gale would change suddenly,

A storm from the N. with an heavy swell from the sea at the S.

denly, and blow from seaward, which did not happen. On the arrival of several ships soon after, the cause of the heavy swell rolling into the entrance of the river was ascertained; for these ships experienced a strong gale from southward, which brought them within about thirty leagues of the entrance of the river. This strong gale from the southward had forced a heavy swell greatly beyond its limit, although this swell must have met with great resistance from the strong northerly wind blowing against it.

Occasioned by a contrary gale.

Barometrical observations.

A long account from Capt. Flinders was recently read at the meetings of the Royal Society, descriptive of barometrical observations, made on the coast of New South Wales, &c. It appears that the mercury continued at greater heights with the wind from the sea, than with land winds, on the coast of New South Wales. I have sometimes observed the same effect in other places, particularly in June 1803 and July 1804, on approaching the coast of Cochinchina. In passing from Sincapour Strait to this coast in these months, with the regular southerly wind, the mercury performed the motions of two elevations and two depressions regularly, during the twenty-four hours; but fell ten hundred parts of an inch each time (suddenly) when we came near the land. At both times the wind from the sea abated, and was replaced with squalls from this alpine country. There was much vapour over the land both times; accompanied with vivid lightning.

The Mercury depressed by squally land breezes.

But not by regular land breezes.

During the fair weather season on the coast of Malabar, when regular land and sea breezes were daily experienced, the mercury in barometers was not depressed by these land breezes; but appeared equally high as when the breezes prevailed from the sea:—The two elevations and depressions of the mercury were continued every twenty-four hours on this coast; although not in equal quantity, as in a steady wind at a considerable distance from land.

III.

A Third Series of Experiments on an Artificial Substance, which possesses the principal characteristic Properties of Tannin; with some Remarks on Coal. By CHARLES HATCHETT, *Esq. F.R.S.*[*]

§ I.

Experiments, &c. on an artificial substance having the character of tanning matter.

IN my former papers upon this subject, some account has been given of the effects produced by sulphuric acid upon turpentine, resin, and camphor; and I shall now state the results of other experiments made with the same acid upon a great number of the resins, balsams, gum resins, and gums, the greater part of which, afforded that modification of the artificial tanning substance, which for the sake of distinction, I have in the preceding papers denominated the third variety.

The process was simple digestion in sulphuric acid, after which, the residuum was well edulcorated, and was then digested into alcohol. This was separated by distillation, the dry substance which remained was infused in cold distilled water, and the portion dissolved, was examined by solution of isinglass, muriate of tin, acetite of lead, and sulphate of iron.

Much sulphureous acid, carbonic acid, several of the vegetable acids, particularly benzoic acid, (when the balsams were employed,) and apparently water, were produced during the operation; but in this paper I shall only notice two of the products, namely, the tanning substance and the coal.

The sulphuric acid almost immediately dissolved the resins, and formed transparent brown solutions, which progressively became black.

The same effect was produced on most of the other substances, but the solutions of the balsams and of guaiacum were at first of a deep crimson, slightly inclining to brown.

Caoutchouc and elastic bitumen were not dissolved, but

* Philos. Trans. 1806. For the former papers see our Journal.

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but after having been digested for more than two months, were only superficially carbonized.

The gums and the saccharine substances required many evaporations and filtrations before the whole of their carbonaceous residua could be obtained.

These were the principal effects observed during the experiments, and I have stated them in this manner, that tedious and useless repetitions may be avoided.

§ II.

Turpentine, common resin, elemi, tacamahac, mastich, copaiba, copal, camphor, benzoin, balsam of Tolu, balsam of Peru, asa fœtida, and amber, yielded an abundance of the tanning substance.

Oil of turpentine also afforded much of it; asphaltum yielded a small portion; some slight traces of it were even obtained from gum arabic and tragacanth; but none was produced by guaiacum, dragon's blood, myrrh, gum ammoniac, olibanum, gamboge, caoutchouc, elastic bitumen, liquorice, and manna. I am persuaded, however, that many of these would have afforded the tanning substance had not the digestion been of too long a duration.

Olive oil was partly converted into the above mentioned substance, and also linseed oil, wax, and animal fat; but the three last appear to merit some attention.

Linseed Oil.

This oil with sulphuric acid very soon formed a thick blackish-brown liquid, which after being long digested in a sand-bath, was still partly soluble in cold water, and passed the filter. This solution precipitated gelatine; the residuum was a tough black substance, which became hard on exposure to air. A great part was soluble on alcohol, and formed a brown liquid, which became turbid by the addition of water. When this was evaporated, a brown substance remained, which was partially dissolved by cold water, and the solution thus formed, was rendered turbid by gelatine.

The undissolved portion left by the alcohol, was of a blackish-brown; it was soft and tenacious, and appeared to retain many of the properties of an inspissated fat oil.

Bleached Wax.

That which was employed in this experiment, was the white

white wax of the shops, which is sold in the form of small round cakes. It formed with sulphuric acid a thick black magma, and was not acted upon by cold distilled water when washed with it upon a filter. Upon being digested with alcohol in a sand-bath, a brownish solution was formed, which upon cooling became very turbid, and appeared as if filled with a white flocculent substance. The same operation was repeated with different portions of alcohol until this ceased to act. The whole of the solutions in alcohol were then mixed, a large quantity of distilled water was added, and the alcohol was separated by distillation.

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On the surface of the remaining liquor, when cold, a white crust was formed, which being separated, was found to possess the properties of spermaceti, and weighed 18 grains. The filtrated liquor was then evaporated to a small quantity, became of a pale brown colour, and was rendered turbid by solution of isinglass.

Animal Fat.

This experiment was made upon the kidney fat of veal, but I cannot take upon me to assert that the results would have been the same with every kind of fat. One hundred grains of it with one ounce of concentrated sulphuric acid, after some time, formed a blackish soft mass: a second ounce of sulphuric acid was then added, and the whole was digested and occasionally heated during nearly three months. Six ounces of distilled water were poured upon the black pulpy mass, and formed a thick uniform liquid, which, after digestion for six or seven days, was when cold filtrated. The liquor which passed was of a brown colour, and upon evaporation became black, leaving a considerable portion of a blackish substance upon the filter, which was added to that which had been collected by the first filtration. The whole was washed with cold water, which passed colourless. Boiling water was then poured upon the filter, by which a considerable portion was rapidly dissolved, and a brownish-black solution was formed, which copiously precipitated gelatine.

The residuum on the filter was then dried, and being collected, was digested in alcohol, which dissolved the greater part.

The solution in alcohol was filtrated, but (apparently by

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by the effect of air) a considerable deposit was formed on the filter, which was again dissolved by alcohol. Water rendered the solution turbid, and a black light flaky substance, which weighed 41 grains, remained upon the filter. The filtrated liquor was then evaporated, and left a grayish-black substance, which weighed 30 grains. This last substance was highly inflammable, and when burned, emitted a very peculiar odour, resembling partly that of fat and partly that of asphaltum. It easily melted, and also immediately dissolved in cold alcohol, from which, like the resinous substances, it was precipitated by water.

The black light flaky residuum, which weighed 41 grains, was found to consist partly of the substance above mentioned and partly of coal, but the proportion of this last was not ascertained.

Coagulated albumen and prepared muscular fibre were also separately exposed to the action of sulphuric acid in the manner above described, but did not afford any substance by which gelatine could be precipitated, coal being the only product which remained.

Almost every one of the bodies which have been employed in these experiments, seem to be in some measure different in respect to the progressive effects produced upon them by sulphuric acid; and all other circumstances being similar, there appears to be a certain period of the process when the production of the tanning substance has arrived at its maximum, after which, a gradual diminution of it takes place, and at length total destruction. These effects are produced at different periods, according to the substance which may be the subject of the experiment, and therefore it is impossible at present to state the utmost quantity of the tanning substance which, under equal circumstances, may be obtained from each of the resins, balsams, &c.

The tanning substance appears to be always the same, whether obtained from turpentine, or common resin, or from the balsams, or from asa fœtida, or camphor, or indeed from any of the bodies which have been enumerated; its effects on the different reagents are similar; by the addition of a small portion of nitric acid, and subsequent evaporation, it is converted into that which I have called the first variety; or if digested with sulphuric acid, it is speedily

speedily destroyed, and becomes mere coal. In the latter case, therefore, the same agent which at first produced it becomes at length the cause of its destruction, and thus we find, that although a tanning substance may be obtained from resinous and other bodies by means of sulphuric and by nitric acid, yet in the former case the product is variable, and is formed at or about the mean period of the operation, whilst the latter is an ultimate and invariable effect, beyond which, no apparent change can be produced by any continuation of the process *.

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§ III.

I have already stated, that caoutchouc, and elastic bitumen, were only superficially acted upon when digested for a very long time in sulphuric acid; and it is remarkable, that these substances, which in their external characters so much resemble each other, should be similar in their habits when exposed to the effects of this acid: for, unlike the resins and most of the other bodies which were subjected to the preceding experiments, and which were almost immediately dissolved when the acid was poured upon them, these on the contrary remained undissolved, and only became partially carbonized on their surfaces. Even nitric acid does not so rapidly effect a change in the elastic bitumen as it does when applied to the other bituminous substances.

1.

One hundred grains of pure soft elastic bitumen were digested during three weeks in one ounce of nitric acid, diluted with an equal quantity of water; a tough and slightly elastic orange-coloured mass then remained. Another ounce of the acid, not diluted, was poured upon this mass, and the digestion was continued until the whole was evaporated. The residuum was tenacious, and of the colour above mentioned. Water partially dissolved it, and formed a deep yellow liquid, which copiously precipitated gelatine, and possessed the other properties of the tanning substance which is produced from the resins, &c. by nitric acid.

* In the former Papers upon this subject I have observed, that the tanning substance produced by sulphuric acid, is very inferior in energy to that, which is formed by nitric acid.

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An orange-coloured mass still remained, which was speedily dissolved by alcohol, and was precipitated from it by a large addition of water.

This substance in many of its properties resembled the resins, but in others, seemed to approach those which characterize the vegetable extractive matter. It appeared to be similar to that which has been cursorily mentioned in my first paper, and which was obtained from many of the pit-coals and bitumens when treated with nitric acid. I have since paid more attention to this substance during the following experiments:

Kilkenny coal was digested with nitric acid, and progressively, although with difficulty, was converted into that variety of the tanning substance which has so often been mentioned. Similar experiments were made on the same sort of coal from Wales, which was given to me by my friend Mr. Tennant, as well as upon a coal sent to me by Professor Woodhouse, which was from Pensylvania, and is there called Leigh high coal. All of these were converted into the tanning substance, but they did not yield any product similar to that obtained from the elastic bitumen.

The contrary however happened when the common pit-coal, or Cannel coal, or asphaltum, were employed. For when these were treated in the way which has been described, and when the digestion was not too long continued, then I obtained from 100 grains of each of the above substances (after the separation of the tanning matter) a residuum as follows:

From 100 grains of the common Newcastle coal 9 grains.
From 100 grains of Cannel coal. ....36 grains.
From 100 grains of pure asphaltum..........37 grains.

The substances thus obtained, were very similar in their external characters, being of a pale brown, approaching to Spanish snuff colour; their internal fracture was dark brown, with a considerable degree of resinous lustre. When exposed to heat they did not easily melt, but as soon as inflamed, they emitted a resinous odour mixed with that of fat oil, and produced a very light coal, much exceeding the bulk of the original substance.

Alcohol completely dissolved them, and if water in a large proportion was added to a saturated solution, a precipitate

precipitate was obtained, but after each precipitation, a portion always remained dissolved by the water, which acted upon the different reagents in a manner similar to the solutions of vegetable extractive matter. The flavour was also bitter, and in some degree aromatic, so that the residua, whether obtained from pit-coal, from Cannel coal, or from asphaltum, seemed to possess properties intermediate between those of resin, and those of the vegetable extractive substance. They appeared, however, to be removed only by a very few degrees from the tanning substance; for if digested in a small quantity of nitric acid, and subsequently evaporated, they were immediately converted into it; or if digested with sulphuric acid, they speedily became reduced to coal.

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§ IV.

In the 5th Section of my second Paper, some remarks were made on the decoctions obtained from vegetable substances which had been previously roasted; and although (excepting one instance) these decoctions did not afford any permanent precipitate with gelatine, yet I have there stated, that I did not think it right to conclude, that similar decoctions made under certain circumstances, might not occasionally possess those properties which characterize the tanning substances. Moreover I also observed in the same paper, that all of those decoctions, upon the addition of a small portion of nitric acid and subsequent evaporation, became converted into that variety of tanning matter which is produced by the action of nitric acid upon carbonaceous substances. I have since extended these experiments, and shall here give some account of them.

1.

Two hundred grains of the fresh peels of horse chesnuts were digested for about 12 hours in three ounces of distilled water. The liquor was of a pale brown, and formed a slight pale brown precipitate when solution of isinglass was added to it.

2.

Two hundred grains of the same peels were moderately roasted, and being afterwards digested with three ounces of water, formed a dark brown decoction, which was not rendered turbid by gelatine.

3.

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The above mentioned roasted peels, after the termination of the preceding experiment, were added to the remainder of the filtrated liquor. A quarter of an ounce of nitric acid was poured upon the whole, which was then digested and evaporated to dryness. The mass was afterwards infused in water, and a dark reddish-brown liquid was obtained, which copiously precipitated solution of isinglass.

4.

Two hundred grains of horse chesnuts, from which the peels employed in the former experiments had been taken, were bruised, and were digested with three ounces of water. The liquor was turbid, and of a pale red colour. It was filtrated, and some solution of isinglass was added, but without any effect.

5.

Two hundred grains of the same horse chesnuts were moderately roasted, and being treated as above described with water, yielded a dark brown decoction which was not rendered turbid by isinglass.

6.

The horse chesnuts, which had been employed in the preceding experiment with the remaining liquor, were digested with a quarter of an ounce of nitric acid until the whole was become dry. Water was then poured upon it, was digested, and a dark brown liquid was formed, which afforded a considerable precipitate by the addition of solution of isinglass.

From these experiments it appears, that the small portion of tannin which the horse chesnut peels originally contained, was destroyed by the process of roasting; that the brown decoction subsequently obtained from the roasted peels and from the horse chesnuts, did not act upon gelatine; but that these were speedily converted into the artificial tanning substance, by the addition of a small portion of nitric acid and subsequent evaporation

The first preparations of the artificial tanning substance which have been mentioned in the former Papers, were made from coal of different descriptions digested with nitric acid, and as similar products have been obtained by the same acid from various decoctions of roasted vegetable

ble substances, there cannot be any doubt, that vegetable bodies when roasted, yield solutions by digestion in water, which essentially consist of carbon approaching to the state of coal, although not absolutely converted into it, for if so, all solubility in water would cease.

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But coal is apparently nothing more than carbon oxidized to a certain degree, and may be formed by the humid as well as by the dry way.

Examples have been already stated respecting operations in which sulphuric acid has produced this effect, but the same likewise appears to be produced with some modifications, whenever vegetable matter undergoes the putrefactive process; for when this takes place, as in dunghills, &c. a large proportion of the carbon of the original vegetable substances appears to be combined with oxygen sufficient to communicate to it many of the properties of coal, whilst the compound nevertheless is capable of being dissolved by water with the most perfect facility.

It must not however be understood that by this process all the other elementary principles are separated, so that only the carbon remains combined with oxygen, but merely, that the other principles are so far diminished, that these, namely, carbon and oxygen, predominate in a state approaching to coal, although soluble in water.

Such solutions, I have every reason to believe, are nearly similar to those afforded by vegetable substances which have been previously roasted, and althongh I have examined but a few of them, yet I shall relate some experiments which I have lately made on the peels of walnuts.

It is well known that when these are kept in small heaps for a short time, they become soft, and break down into a black mass, which affords a brownish-black liquor. On these I therefore made the following experiments:

1.

About one ounce of walnut peels, which were become soft and black, was digested in water.

A dark brown liquor was thus formed, and being filtrated, was examined by a solution of isinglass, but not any apparent effect was produced.

2.

On an equal quantity of walnut peels, in the same soft black

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black state, a small portion of nitric acid was poured, and after being digested for about five hours, the whole was evaporated to dryness. The residuum was of a brownish orange colour, and yielded a similar coloured solution to water when digested with it. This was filtrated, and upon the addition of solution of isinglass, became turbid, and deposited a tough precipitate, which was not dissolved by boiling water.

3.

Another portion of the walnut peels was moderately roasted, and was then digested in water; the brown solution was filtrated, and formed a slight precipitate with gelatine.

4.

On the residuum of the last experiment, a small quantity of nitric acid was poured, some water was then added, the whole was digested during about five hours, and until it became perfectly dry.

Water formed with this a brown liquor, which yielded a very abundant precipitate by the addition of dissolved isinglass.

Upon these experiments we may remark, that the solution in the first instance contained carbon in a state approaching to coal, for when treated with nitric acid in the second experiment, a portion (although small) was produced of the same tanning substance which is formed from the different kinds of coal by nitric acid.

The third experiment appears to shew, that a small quantity of a substance approaching to tannin was produced by the simple process of roasting; and the fourth experiment corroborates those already described, in which the artificial tanning matter was copiously produced, whenever roasted vegetable substances were treated with nitric acid.

In respect to vegetable substances, especially those which contain tannin, I shall here relate a few other experiments.

It has been remarked in my second Paper, (p. 288,) that the tannin of galls was immediately destroyed by nitric acid. Since that time, I have made the following additional experiments:

One

1.

One hundred grains of galls reduced to powder were infused with four ounces of water, and part of the infusion upon the addition of solution of isinglass afforded (as usual) a copious precipitate of a brownish-white colour.

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A quarter of an ounce of nitric acid was added to one ounce of the above infusion, which then, was not in any manner affected by the dissolved isinglass.

2.

One hundred grains of the same galls were slightly roasted, and being digested with four ounces of water, formed a brown liquor, which was filtrated.

Solution of isinglass was then added to a part of the above liquor, and produced a precipitate not very unlike the former, but much less in quantity.

After this, a quarter of an ounce of nitric acid was added to one ounce of the same liquor, and some dissolved isinglass was subsequently poured into it, by which it was rendered turbid, and a small portion of a dark brown precipitate was produced, resembling that which is commonly afforded by the artificial tanning substance.

3.

The remainder of the above mentioned liquor, with the residuum of the roasted galls, were digested with a quarter of an ounce of nitric acid until the whole had become dry. Water was then poured upon it, and formed a dark brown solution, which yielded a copious brown precipitate by the addition of dissolved isinglass.

From these experiments on galls it appears, that the natural tannin contained in them is destroyed by nitric acid; that the tannin is also diminished, (and I may add,) is ultimately destroyed by the process of roasting; that when galls have not been so far roasted as to destroy the whole of the tannin, then the remainder of this seems to be destroyed by the addition of nitric acid, whilst at the same time a small portion of the artificial tanning substance is produced; and that this last is always plentifully afforded by roasted galls when digested with nitric acid, similar to other vegetable bodies when thus treated.

These remarks are also partly confirmed by the following experiments upon oak bark.

1.

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Two hundred grains of oak bark, reduced into very small fragments, were infused in about four ounces of water, after which the infusion was examined by dissolved isinglass, and yielded a considerable precipitate.

2.

Two hundred grains of the same sort of bark were slightly roasted, and afterwards digested in water; a much darker coloured liquor was obtained than in the former case; but although it afforded precipitates by the addition of the muriate of tin, acetite of lead, and sulphate of iron, yet not the smallest effect was produced by solution of isinglass.

3.

The residuum, with the remaining part of the above mentioned liquor, was then digested with a small portion of nitric acid; this was completely evaporated, and a brown solution was formed by water, which abundantly precipitated gelatine.

4.

One ounce of oak bark, reduced into very small fragments, was repeatedly digested in different portions of water until the whole of its tannin was extracted. The residuum or exhausted bark (as it is called by the tanners) was dried, and was afterwards moderately roasted. It was then moistened with diluted nitric acid, which was evaporated in a heat not much exceeding 300° until the bark was become perfectly dry. This was digested in water, and speedily formed a yellowish-brown liquor, which abundantly precipitated gelatine.

5.

The bark, which after being exhausted of its natural tannin, had thus afforded the artificial tanning substance, was repeatedly treated with water until the whole of this last was extracted. The bark was then again slightly roasted, was again moistened with nitric acid, and was gently heated and dried as before. Water being poured on it and digested, formed a brown solution, which copiously precipitated gelatine.

The

6

The whole of the artificial tanning substance was extracted by different portions of water, and the remainder of the bark thus exhausted, was again treated in the manner above described, and again afforded a considerable quantity of the tanning substance, so that these processes evidently might have been continued until the whole of the bark had been converted into it.

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This might also have been accomplished, if in the first instance, the exhausted bark had been converted into charcoal, and digested in nitric acid, as described in my first Paper; but then, the effects would have been more slowly produced, and much more nitric acid would have been consumed. I am now therefore fully convinced, not only by the results of the experiments related in this Paper; but also by many others which it would have been superfluous to have stated, that the most speedy and most economical of all the processes which I have described, is that of treating roasted vegetable substances in the way which has been mentioned, and considering that all refuse vegetable matter may be thus converted into a tanning substance by means the most simple, and without any expensive apparatus, I cannot help entertaining much hope, that eventually this discovery will be productive of some real public advantage.

§ V.

In my first Paper I have remarked, that I suspected the tannin of the peat moors to have been produced during the imperfect carbonization of the original vegetable substances. Whether this has been the case, or whether the tannin has at times been afforded by heath and other vegetables growing upon or near the peat, still appears to me to be uncertain; but whatever may be the origin, I never have yet been able to detect any tanning substance in peat, although I have examined a considerable number of varieties, some from Berkshire, and many from Lancashire, which were obligingly sent to me for this purpose by my friend John Walker, Esq. F. R. S. Mr. Jameson has also made the same observation,* so that there

* An Outline of the Mineralogy of the Shetland Islands, &c. 8vo. edition, p. 174.

 cannot

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cannot be any doubt (whatever the origin of the tanning matter may have been) that it has speedily been extracted and drained from the substances which first contained it. This effect is a natural consequence of the great facility with which tannin is dissolved by water, and extends even to the most solid vegetable bodies; I shall here give an example.

In the Philosophical Transactions for 1799, Dr. CORREA DE SERRA has given an account of a submarine forest at Sutton, on the coast of Lincolnshire, where submerged vegetables are found in great abundance, including trees of different descriptions, especially birch, fir, and oak. At the time when I was engaged in those experiments on the Bovey coal, and other substances of a similar nature, which have been printed in the Philosophical Transactions for 1804, Sir JOSEPH BANKS had the goodness to send me a piece of the oak, which was perfect in all of its vegetable characters, and did not appear to have suffered any change excepting, that it was harder, and of a darker colour than recent oak wood. From some experiments which I then made, I found, that after incineration it afforded potash, similar to the recent wood, and contrary to substances like the Bovey coal, which retain the vegetable external characters, although imperfectly converted into coal*.

In the course of my experiments on tannin, I reduced about an ounce of this submerged oak into shavings, and digested them in water. A brown decoction was formed, which with muriate of tin afforded a pale brown precipitate; with acetite of lead, a precipitate of a deeper brown; with sulphate of iron, a copious brownish-black precipitate; but with solution of isinglass not any effect was produced.

The tannin of this oak wood, had therefore either been separated by solution, or had been decomposed; so that the only substance which remained capable of being dissolved by water, was the extractive matter. This last, in the present case was most probably the original extractive matter of the oak, but in some other instances, (such, for example, as that which was found in the alder leaves

* Phil. Trans. for 1804, p. 399.

contained

contained in the Iceland schistus.*) I am much inclined to believe, that an extractive substance of secondary formation, if I may be permitted to employ such a term, is produced during the process of carbonization. If a substance, therefore, so compact and solid as oak timber can by long submersion, be deprived of its tannin, it naturally follows that the same effect must be more speedily produced by the action of water on the smaller vegetable bodies, which present an extensive surface, and also on porous and bibulous substances such as peat.

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But although peat, as I have already observed, does not contain any tannin, yet the imperfect carbonization which it has undergone, renders it like the roasted ligneous bodies, peculiarly susceptible of being converted into the artificial tanning substance when exposed to the action of nitric acid. It would be useless to enter into a detail of the different experiments which I have made upon it, as they were similar to those already related, and I shall therefore only here state, that when seven ounces of well dried peat had been twice moistened, and digested with diluted nitric acid, (to the amount of rather more than two ounces,) and subsequently dried, I obtained by water a solution of the artificial tanning substance, which when evaporated to dryness weighed two ounces. I am convinced, that much more might have been obtained from the residuum of the peat, had I thought proper to have repeated the operation; and I am also certain, that less nitric acid would have been sufficient, had the process been conducted in close vessels, and with other economical precautions, which at that time, were for the sake of expedition and convenience omitted.

§ VI.

It has been generally stated, even by modern chemists, that the acids act but little, if at all, upon resinous substances.

The contrary has however been proved, not only in the three Papers upon the present subject, but also in some others which I have formerly had the honour to lay before this learned society.

In my experiments on lac, printed in the Phil. Trans. for

* Phil. Trans. for 1806, p. 391.

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for 1804, p. 208, I have particularly endeavoured to shew, how powerfully the acetic acid acts upon resin, gluten, and some other substances; so that it may justly be regarded, as a valuable agent in the chemical analysis of vegetable bodies. In this point of view, it is as a solvent to be the more highly appreciated, because it appears to dissolve the resins, &c. without affecting their respective qualities, and thus by proper precipitants, these substances may be separated from it pure and unaltered.

I am induced therefore to consider acetic acid to be the true acid solvent of the resinous substances, as it dissolves them speedily, without producing any apparent subsequent change in their natural properties.

Sulphuric acid also, almost immediately dissolves the resins, balsams, &c. and forms transparent brown or sometimes crimson solutions, the latter colour being most commonly characteristic of the balsams.

These solutions, however, are different from those made in the acetic acid, by not being permanent, for from the moment when the solution is completed, progressive alterations appear to be produced in the body which is dissolved; thus turpentine is almost immediately converted into resin, then into the third variety of the tanning substance, and lastly into coal.

Without being under the necessity of adducing other examples, we may therefore state sulphuric acid to be a solvent of the resinous substances, but which continues afterwards to act on their principles, so as to decompose them, coal being the ultimate product.

Nitric acid, as I have shewn in the course of these Papers, and likewise on some former occasions, dissolves the resins, but the progress of its effects seems to be conversely that of sulphuric acid; in the latter case, solution precedes decomposition; but when nitric acid is employed, decomposition to a certain degree precedes solution; for it at first converts the resins into a pale orange coloured brittle porous substance, then into a product, which apparently possesses the intermediate characters of vegetable extractive matter and of resin, and lastly, this is converted into the first variety of the tanning substance, beyond which I have not been able to effect any change.

As

As coal therefore appears to be the ultimate effect produced by sulphuric acid upon the resinous bodies, so does the first variety of the tanning substance seem to be the terminating product afforded by the same when acted upon by nitric acid. This effect of nitric acid has been already amply discussed, neither does it appear necessary that I should here repeat the remarks which have been made on some of the simultaneous products, such as the vegetable acids; but amongst the effects produced by sulphuric acid, the coal which is formed seems to merit some attention.

[The remainder in our next.]

IV.

On the Force of Percussion. By William Hyde Wollaston, *M.D. Sec. R.S. Being the Bakerian Lecture which was read before the Royal Society in the month of November last* *.

Concise statement of the dispute concerning the forces of bodies in motion.

When different bodies move with the same velocity, it is universally agreed that the forces, which they can exert against any obstacle opposed to them, are in proportion to the quantities of matter contained in the bodies respectively. But, when equal bodies move with unequal velocities, the estimation of their forces has been a subject of dispute between different classes of philosophers. Leibnitz and his followers have maintained that the forces of bodies are as the masses multiplied into the *squares* of their velocities, (a multiple to which I shall for conciseness give the name of *impetus*); while those, who are considered as Newtonians, conceive that the forces are in the *simple ratio* of the velocities, and consequently as the *momentum* or *quantitas motus*, a name given by Newton to the multiple of the velocity of a body simply taken into its quantity of matter.

It cannot be expected that at this time any new experiment should be thought of, by which the controversy can be decided, since the most simple experiments that

* From the Phil. Trans. for 1806.

have

have already been appealed to by either party have received different interpretations from their opponents, although the facts were admitted.

The Newtonian explanation of the third law of motion is not contrary to the followers of Leibnitz.

My object in the present lecture is to consider which of these opinions respecting the force exerted by moving bodies is most conformable to the usual meaning of that word, and to shew that the explanation given by NEWTON of the third law of motion is in no respect favourable to those who in their view of this question have been called NEWTONIANS.

If bodies were made to act upon each other under the circumstances which I am abont to describe, the leading phænomena would occur, which afford the grounds of reasoning on either side.

Statement of the leading phenomenon.

Let a ball of clay or of any other soft and wholly inelastic substance be suspended at rest, but free to move in any direction with the slightest impulse; and let there be two pegs similar and equal in every respect inserted slightly into its opposite sides. Let there be also two other bodies, A and B, of any magnitude, which are to each other in the proportion of 2 to 1; suspended in such a position, that when perfectly at rest they shall be in contact with the extremities of the opposite pegs without pressing against them. Now if these bodies were made to swing with motions so adapted that in falling from heights in the proportion of 1 to 4 they might strike at the same instant against the pegs opposite to them, the ball of clay would not be moved from its place to either side; nevertheless the peg impelled by the smaller body B, which has the double velocity, would be found to have penetrated twice as far as the peg impelled by A.

It is unnecessary to make the experiment precisely as here stated, since the results are admitted as facts by both parties; but upon these facts they reason differently.

Inferences concerning the forces as deduced by each party.

One side observing that the ball of clay remains unmoved, considers the proof indisputable that the action of the body A is equal to that of B, and that their forces are properly measured by their momenta, which are equal, because their velocities are in the simple inverse ratio of the bodies. Their opponents think it equally proved by the unequal depths to which the pegs have penetrated, that the

the causes of these effects are unequal, as they find to be the case in their estimation of the forces by the squares of the velocities.

One party is satisfied that equal *momenta* can resist equal pressures during the same *time;* the other party attend to the *spaces* through which the same moving force is exerted, and finding them in the proportion of 2 to 1, are convinced that the *vis viva* of a body in motion is justly estimated by its magnitude and the square of its velocity jointly.

In the one consideration the time of action is attended to; in the other the result or work performed. — This is practically more useful.

The former conception of a quantity dependent on the continuance of a given *vis motrix* for a certain *time* may have its use, when correctly applied, in certain philosophical considerations; but the latter idea of a quantity resulting from the same force exerted through a determinate *space* is of greater practical utility, as it occurs daily in the usual occupations of men; since any quantity of work performed is always appreciated by the extent of effect resulting from their exertions; for it is well known, that the raising any great weight 40 feet would require 4 times as much labour as would be required to raise an equal weight to the height of 10 feet, and that in its slow descent the former would produce 4 times the effect of the latter in continuing the motion of any kind of machine. Moreover, if the weights so raised were suffered to fall freely through the heights that have been ascended by means of 4 and 1 minute's labour, the velocities acquired would be to the ratio of 2 to 1, and the squares of the velocities in proportion to the quantities of labour from which they originated, or as 4 to 1; and if the forces acquired by their descent were employed in driving piles, their more sudden effects produced would be found to be in that same ratio.

It has been called mechanic force,

This species of force has been, first by BERNOUILLI and afterwards by SMEATON, very aptly denominated mechanic force; and when by force of percussion is meant the quantity of mechanic force possessed by a body in motion, to be estimated by its quantity of mechanic effect, I apprehend it cannot be controverted that it is in proportion to the magnitude of the body and to the square of its velocity jointly.

But

and is no where treated of by Newton.

But of this quantity of force NEWTON no where treats, and has accordingly given no definition of it. If, after defining what he meant by the *quantitas acceleratrix*, and *quantitas motrix*, he had had occasion to convey an equally distinct idea of the *quantitas mechanica* resulting from the continued action of any force, he might, not improbably, have proceeded conformably to the definition given by SMEATON, and have added

——quantitas mechanica est mensura proportionalis spatio per quod data vis motrix exercetur;

or, if speaking with reference to the accumulated energy communicated to a body in motion,

——proportionalis quadrato velocitatis quam in dato corpore generat.

But, if we attend to the words of his preface to the first edition of his *Principia*, he evidently had no need of such a definition;

"Nos autem non artibus sed philosophiæ consulentes, "deque potentiis non manualibus sed naturalibus scri- "bentes," &c.

And again, nearly to the same effect in the *Scholium*, which follows the laws of motion, "Cæterum mechani- "cam tractare non est hujus instituti."

Newton speaks of pressures, & not of percussion.

In the third law of motion he has on the contrary been supposed to speak of this force from an ambiguity in the signification of the words *actio* and *reactio*. By these, however, NEWTON certainly meant a mere *vis motrix* or pressure, as he himself explains them. "Quicquid pre- "mit vel trahit alterum, tantundem ab eo premitur vel "trahitur. Si quis lapidem digito premit, premitur et "hujus digitus a lapide," &c. The same meaning is equally evident from his demonstration of the third corollary to the laws, in which he asserts that the *quantitas motûs* of two or more bodies estimated in any given direction is not altered by their action upon each other. The demonstration begins thus:

"Etenim actio eique contraria reactio æquales sunt "per legem tertiam, ideoque per legem secundam æqua- "les in motibus efficient mutationes versus contrarias "partes." Now, if he had considered the third law as implying equality of more than mere moving forces, there could

could have been no occasion to refer to the second law, with a view thence to deduce the equality of momenta produced.

A more complex consideration of the third law.

Some authors however have interpreted the third law differently, and accordingly have expressed a difficulty in comprehending the simple illustration given by NEWTON. When they say that action is equal to reaction, they mean not only that the instantaneous intensity of the moving forces, or pressures opposed to each other, are necessarily equal, but conceive also a species of accumulated force residing in a moving body, which is capable of resisting pressure during a *time* that is proportional to its momentum or quantitas motus.

If it be of any real utility to give the name of force to this complex idea of *vis motrix* extended through time, as well as that of *momentum* to its effects when unresisted, it would be requisite to distinguish this force always by some such appellation as *momental* force; for it is to be apprehended that for want of this distinction many writers themselves, and it is certain that many readers of disquisitions on this subject have confounded and compared together *vis motrix*, *momentum*, and *vis mechanica*: quantities, that are all of them totally dissimilar, and bear no more comparison to each other, than lines to surfaces, or surfaces to solids.

The momentum of bodies is seldom to be considered in practical mechanics.

In practical mechanics, however, it is at least very rarely that the *momentum* of bodies is in any degree an object of consideration: the strength of machinery being in every case to be adapted to the *quantitas motrix*, and the extent and value of the effect to be produced depending upon the *quantitas mechanica* of the force applied, or in other words to the space through which a given vis motrix is exerted.

Smeaton has well treated of mechanic force.

The comparative velocities given by different quantities of mechanic force to bodies of equal or unequal magnitude, have been so distinctly treated of by SMEATON, in a series of most direct experiments*, that it would be a needless waste of time to reconsider them in this place. So also, on the contrary, the quantities of extended mechanic

* Phil. Trans. Vol. LXVI. 450.

chanic effect producible by bodies moving with different quantities of impetus have been as clearly traced by the same accurate experimentalist*.

Farther considerations respecting forces.

But there is one view, in which the comparative forces of impact of different bodies was not examined by SMEATON, and it may be worth while to shew that when the whole energy of a body A is employed without loss in giving velocity to a second body B, the *impetus* which B receives is in all cases equal to that of A, and the force transferred to B, or by it to any third body C, (if also communicated without loss, and duly estimated as a mechanic force,) is always equal to that from which it originated.

As the simplest case of entire transfer, the body A may be supposed to act upon B in a direct line through the medium of a light spring, so contrived that the spring is prevented by a ratchet from returning in the direction towards A, but expands again entirely in the direction towards B, and by that means exerts the whole force which had been wound up by the action of A, in giving motion to B alone. In this case, since the moving force of the spring is the same upon each of the bodies, the accelerating force acting upon B at each point is to the retarding force opposed to A at the corresponding points in the reciprocal ratio of the bodies, and the squares of the velocities produced and destroyed by its action through a given space will consequently be in that same ratio. The momentum, which is in the simple reciprocal ratio of the bodies, might consequently be increased at pleasure by the means proposed, in the subduplicate ratio of the bodies employed; and if momentum were an efficient force capable of reproducing itself, and of overcoming friction in proportion to its estimated magnitude, the additional force acquired by such a means of increase, might be employed for counteracting the usual resistances, and perpetual motion would be easily effected. But since the *impetus* remains unaltered, it is evident that the utmost which the body B could effect in return would be the reproduction of A's velocity, and restitution of its

* Phil. Trans. Vol. LXXII. 337.

entire

entire mechanic force neither increased nor diminished, excepting by the necessary imperfection of machinery. The possibility of perpetual motion is consequently inconsistent with those principles which measure the quantity of force by the quantity of its extended effect, or by the square of the velocity which it can produce.

Considerations respecting mechanic force.

In estimating the utmost effect which one body can produce upon another at rest, the same result is obtained by employing *impetus* as ascensional force, according to HUYGENS; for if the body A were allowed to ascend to the height due to its velocity, and if by any simple mechanical contrivance of a lever or otherwise the body B were to be raised by the descent of A, it is well known that the heights of ascent would be reciprocally as the bodies; and consequently that the *square* of the velocity to be acquired by free descent of B would be in that ratio, and the quantity of mechanic force would be preserved as before unaltered.

It may be of use also to consider another application of the same energy, and to shew more generally that the same quantity of total effect would be the consequence not only of direct action of bodies upon each other, but also of their indirect action through the medium of any mechanical advantage or disadvantage; although the time of action might by that means be increased or decreased in any desired proportion. For instance, if the body supposed to be in motion were to act by means of a lever upon a spring placed at a certain distance from the centre of motion, the retarding force opposed to it would be inversely as the distance of the body from the centre; and since the space through which the body would move to lose its whole velocity would be reciprocally as the retarding force, the angular motion of the lever and space through which the spring must bend, would be the same, at whatever point of the lever the body acted. And conversely, the reaction of the spring upon any other body B, would in all positions communicate to it the same velocity.

It may be remarked, however, that the times in which these total effects are produced may be varied at pleasure in proportion to the distances at which the bodies are placed

Considerations respecting mechanic force.

placed from the centre of motion; and it should not pass unobserved that, although the intensity of any *vis motrix* is increased by being placed at what is called a mechanical advantage, yet on the contrary, any quantity of mechanic force is not liable to either increase or diminution by any such variation in the mode of its application.

Since we can by means of any mechanic force consisting of a *vis motrix* exerted through a given *space*, give motion to a body for the purpose of employing its *impetus* for the production of any sudden effect, or can, on the contrary, occasion a moving body to ascend, and thus resolve its *impetus* into a moving force ready to exert itself through a determinate space of descent, and capable of producing precisely the same quantity of mechanic effect as before, the force depending on *impetus* may justly be said to be of the same kind as any other mechanic force, and they may be strictly compared as to quantity.

In this manner we may even compare the force of a body in motion to the same kind of force contained in a given quantity of gunpowder, and may say that we have the same quantity of mechanic force at command whether we have 1lb. of powder, which by its expansion could give to 1 ton weight a velocity sufficient to raise it through 40 feet, or the weight actually raised to that height and ready to be let down gradually, or the same weight possessing its original velocity to be employed in any sudden exertion.

By making use of the same measure as in the former cases, a distinct expression is likewise obtained for the quantity of mechanic force given to a steam-engine by any quantity of coals; and we are enabled to make a comparison of its effect with the quantity of work that one or more horses may have performed in a day, each being expressed by the space through which a given moving force is exerted. In the case of animal exertion however, considerable uncertainty always prevails in consequence of the unequal powers of animals of the same species, and varying vigour of the same animal. The information which I have received in reply to inquiries respecting the weights raised in one hour by horses in different

different situations, has varied as far as from 6 to 15 tons to the height of 100 feet. But although the rate at which mechanic force is generated may vary, any quantity of work executed is the same, in whatever time it may have been performed.

Considerations respecting mechanic force.

In short, whether we are considering the sources of extended exertion or of accumulated energy, whether we compare the accumulated forces themselves by their gradual or by their sudden effects, the idea of mechanic force in practice is always the same, and is proportional to the *space* through which any moving force is exerted or overcome, or to the *square* of the velocity of a body in which such force is accumulated.

V.

Letter from a Correspondent, affirming, contrary to some Observations in our last Number, that objects can be distinguished by the Human Eye under Water, with additional experiments by the Editor.

London Institution, 22d Aug. 1806.

To Mr. Nicholson.

Sir,

I have just laid down your Journal for the present month, and feel myself not a little surprised at your dissertation on swimming, and your observations on what Dr. Franklin has written on that subject.

Your objection to the Doctor's mode of giving confidence to those who wish to acquire the art, appears to me not founded on fact, at least not general fact; you seem to think the Doctor's plan like the senatûs consultum of the mice, a very good one, but quite impracticable: now I find two very learned men differing upon a particular ascertainable point, ascertainable to any one who has eyes formed without some very uncommon defect, and who has, at the same time, courage enough to plunge his head under *clear* water. (For the sharpest sighted man cannot see in water, where the light is shut out by mud floating in it, or by the sides of a vessel that is too small to admit its rays, any more than in a room where light is shut out by the window shutters being closed).

Remark on Dr Franklin's supposition that objects can be distinguished by divers.

I have reason to conclude that both yourself and Dr. Franklin are swimmers, although by your own confession you cannot be a very adept diver; you could not at that time however, when you vainly endeavoured to regain your silver buckle, shining as the substance was, in four feet water.

Conjecture that W. N. is misled by fancy in concluding that they cannot.

I believe that very studious and scientific men do oftentimes in their studies, hit upon some fanciful theoretical point, upon which they build, without that solid base which their own knowledge would easily discover, were they to consult it.

Complimentary apology.

I ought, I am well aware, to beg pardon when I presumed to differ from a man so learned and of such eminent abilities as yourself; and were it a point of theory, which depended on the mind's eye, I should, if I did not see the subject in the same light as yourself, impute it to a mental opthalmia on my part, and not think of offering an opinion in opposition to yours. But in this case, I cannot help thinking there must be some essential difference in the formation of the crystaline humour of your eyes, or in the body of the waters wherein you have tried your experiments; for I have frequently dived, not in the Indies, but no farther off than Eton, in the Thames, in water from six to ten feet deep, for things thrown down to the bottom for the very purpose of diving to bring them out again, and have done it with success; and my schoolfellows doing the same, I could not suppose the property of seeing under water, was peculiar to myself; but as I was never very fond of diving much, I have not of late practised it, yet doubt not I could do the same thing were I now to try.

The author states that he can see very well under water.

I trust you will excuse me for the liberty I have taken in thus troubling you; but I did not think it right that an error should go forth to the world, under the sanction of a name that carries with it such weight.

I am, Sir,
With the greatest respect,
Your very obedient servant,
A DIVER.

P. S. If you should be satisfied of the above mistake, I hope you will mention it in your next number.

Reply by W. N.

THE proper answer to the preceding letter will consist in relating a few experiments which I have just made, along with two of my friends.

Experiment. A glass vessel, two feet deep, was filled with Thames water.

We took neither Oriental nor Batavian waters, but a portion from that venerable stream in which my correspondent and his schoolfellows so successfully exercised their sight. With this we filled a glass cylindrical vessel, 2 feet high and 1 foot wide, standing upon a white (paper) ground. Two pieces of black lead, sawed square, were put into the water, one of which is one inch and a half wide, and the other only half an inch. Both these pieces were very distinctly seen by the eye above the water, but they were not visible to any of us when we looked at them with the face plunged beneath the surface.

A square of an inch and a half scarcely discernable; a smaller square invisible

The larger piece exhibited a darkish hazy appearance, which was very faint and not at all likely to have been noticed, if the attention had not been steadily directed to its known place. The smaller piece did not perceptibly affect the white ground.

Cork square, of an inch, invisible, unless very near.

A square piece of cork was fixed to a bended rod, so that it could be plunged to different depths in the water. At the bottom of the vessel, it was quite invisible to the immersed eye. I could just see that it was square when at the distance of six inches. Others were not satisfied as to its figure, but at a less distance.

A buckle and an egg.

A buckle and an egg were also tried, the former was invisible, and the egg (upon a black ground) was very hazy, confused and indistinct, so that we did not think it could have been found by sight, by a diver.

A convex lens of half an inch focus enabled the eye to see distinctly.

Various convex lenses were applied to the eye, under water, in order to bring the rays to their proper focus on the retina. The lens which answered our purpose, is a double convex crystal glass of half an inch focus in the air (but two inches from under water) when this lens was held close to one of the immersed eyes, the other being shut, the objects at the bottom of the water were distinctly seen. I clearly observed the saw marks on the black

 lead

lead, several small matters adhering to the egg and an air bubble of one twentieth of an inch diameter, which adhered to the buckle.

The eye saw best at first immersion.

We all remarked that the appearance under water, to the naked eye, became less distinct after the eye had been a second or two under the water than at first. I do not apprehend why this should be the case.

Conclusion that the human eye cannot distinguish objects under water.

From the preceding facts as well as from optical considerations, it appears to be established that men and probably all animals which live in the air, are incapable of varying the adjustment of the eye, so as to distinguish objects, with even a very small degree of precision, at a very remarkable distance from that organ immersed in water. Instead however of reasoning, as my correspondent has done in his fourth paragraph, I am disposed to question whether a farther enquiry into the facts, with different individuals, might not shew that some persons may be capable of altering the form of their eyes enough to see imperfectly in the situation we have been contemplating; but I must confess that I do not incline to that opinion.

Another instance of a person swimming at first time.

Since I wrote the paper in my last, I have heard of another well authenticated instance of a man who had never attempted to swim, but who, on the occasion of having fallen from a barge into the Thames, supported himself for a considerable time by striking his hands downwards alternately, until a boat, for which he loudly called, came to his assistance.

VI.

History of the Developement of the Intellect and Moral Conduct of an Infant during the first Twelve Days of its Existence. In a Letter from R. B.

To Mr. NICHOLSON.

SIR,

Introduction to this memoir.

SIX years ago I communicated to a respectable periodical publication, a register of the moral conduct of an infant for the first twelve days of its existence. That work was soon afterwards discontinued, which prevented my communica-

I ting

ting any farther continuation of an history which seems calculated to give instruction to many of those whose researches have been directed towards the habits and progress of the human mind. From the very moderate success of the work alluded to (which was not calculated for the million) I consider my memoir as yet unpublished. If, Mr. Nicholson, it should so far meet your approval, and coincide with the plan of your excellent Journal, as to obtain insertion, I shall take the liberty to communicate the remaining part of my register, which will bring the history to a period when infants are in some measure admitted to and supposed capable of mutual intercourse with persons of more advanced age.

Inquiries made by a parent respecting infants.

I am the father of many children, and consequently it is now a considerable number of years since I experienced the first emotions of a parent. At that period, every movement, every action of the little being of which I was destined to be the protector and guide, were subjects of curiosity and interest. My speculations on its figure, its passions, and the gradual developement of its understanding, were numerous, rapid, and confused. When I went into society, I made inquiries of the age of children in every stage of advancement, and classed their attainments in regular progression for my own use, in estimating what I was to expect from the new object of my regards. One month passed after the other, and my acquisitions of knowledge, both physical and moral, respecting the early stage of our existence, became greater, while my entertainment and surprise were such as, perhaps, can be rightly estimated by none but those who have themselves been parents. After the lapse of two years, I was again a father, but found, with some mortification, that I had nearly forgotten all my stock of knowledge, and that the same series of remarks was again to be made. I then made a few notes; but with so little precision, that they were scarcely of any use, when on a subsequent discussion with an author of high reputation concerning the influence of education, I was desirous of examining the value of the facts which have led me to conclude that our mental powers are originally as different as our physical; and that, at the moment of birth, our structure

and moral habits are so far formed, that a physiognomist would find no difficulty in writing the character of an infant an hour old.

This incident led me to make a fuller register of the manners and progress of my ninth infant; which, however, upon review, I find to be much less precise than I now wish. Such as it is, I shall, however, make it the basis of my present communication.

First efforts of an infant as actually observed

The circumstances of parturition had so far exhausted the infant (a female) that instead of immediate respiration and crying, as commonly happens, the time of thirty seconds elapsed before she breathed; during which she occasionally opened her mouth to the utmost. The respiration commenced with a convulsion of the thorax, or single hiccup; soon after which the funis was divided, and she was delivered to me*. During the following seven or eight minutes, her respiration was several times suspended for an instant; at the end of which, the system was enough recovered to obey the repeated voluntary exertions she made, and she cried freely. Before this time the head had assumed its regular figure, and had entirely lost the elongation produced by the act of parturition.

The following are the observations of physiognomy † made on the day of birth, and abundantly confirmed, as far as the present age of the subject (three years) can show.

Estimate of her character, &c. from her figure and conduct.

The general figure of the head is capacious, regularly oblong, nearly circular behind, and narrowed above the forehead.

* It is not the custom of this country for the father to be present at events of this nature; but I am well convinced that his presence, if he possess firmness, good sense, and affection, is calculated to produce the happiest effects.

† I am very little disposed to apologise for observations of physiognomy. Those who deride them the most, are perpetually making them. That an individual has an intelligent, a stupid, a malignant, a ferocious, a timid, a courageous, &c. look;—that his manner is spirited, dignified, generous, or mean, contemptible, base, sneaking;—these are terms as familiar as any in our language; they are applicable to paintings equally as to men, and consequently indicate lines and shades capable of being copied and systematised. The physiognomist is he who does this with more precision than common observers.

forehead. The forehead is of a good shape, and ends in eye-brows marked with dark hair, the lines considerably horizontal, and supported by bones which terminate well at the temples, and moderately well at the nose. The eyes are dark, and marked with intelligence (that is to say), by the proximity of the edge of the lid to the line of the eye-brow; the line or indenture proceeding from the nose along the upper part of the eye-lid; and other general circumstances, particularly the steady, lively look, and brisk voluntary changes of position produced by the muscles of this organ and those of the neck, at intervals moderately remote. The nose terminates well above, its profile very slightly hooked, the holes large, and the mouth habitually closed. The prominences or lines from the sides of the nose across the cheeks and the neighbouring parts are of the figure which accompanies a placid, affectionate disposition. This is confirmed also by her manner of crying. When she cries from pain, vertical wrinkles appear between the eye-brows; but when from affront, or external inconvenience, the corners of the mouth are depressed. In neither case does she exhibit rage, unless in the extreme of the latter. The lines of the mouth, which is moderately wide, are marked and distinct, and the muscles very accurately disposed to feed either by a vessel or the breast. She fed eagerly and plentifully at fifteen minutes old.

Hence I infer, that she has more than the middle degree of understanding, and is of a calm, placid, though lively and susceptible disposition.

So far proceeds my first note. The others are dated in days of the age of the infant, and relate to points on which Rousseau, and other authors of repute, have written ignorantly. I shall make few remarks, but copy without amendment.

First social acts of infants' attention to the voice.

Second day. Exercise of the hands, legs, and eyes, first engage the attention of infants. By talking in a tone rather musical and uniform, with repetition of the same syllable, and occasional variation, her attention is so much engaged, that she suspends the play of her limbs, and listens very attentively. This was done so frequently in the course of the day as to render her very lively,

lively, and irritably attentive to surrounding objects. It was necessary to give her one drop of tincture of opium, which restored the usual state and disposition for repose.

Voluntary vision.

Fourth day. She can follow an object with the eyes, when her attention is excited, and now and then unequivocally smiles during her observations on surrounding objects.

Expression of pleasure, and articulation.

Fifth day. Her pleasure at viewing luminous and other objects is expressed, not only by the general features, but by efforts of the mouth, protruding the lips, advancing and withdrawing the tongue. She has once or twice pronounced the usual first voluntary articulation of infants, *ku* and *ac-ku*. She is very attentive to language, and answers with smiles, terminating in the word *ac-ku*, and a lively exertion of the legs. She has, apparently

Optic axes adjusted.

for the first time, began to attend to the adjustment of the optic axes, and probably the concomitant focal adjustment of the eye. She is delighted to lie and contemplate the breast at a little distance, after having satisfied her appetite. In this position, as well as when attentive to the kind countenance of the parent who speaks to her, the mouth is protruded and occasionally modified to the indication of pleasure resembling the action of sucking, and all adjustments of the optical axes are tried, from squinting, to the usual very regular position of the eye*.

Notion of falling.

Sixth day. The habits of attention, and answering by smiles, by attempts at articulation, and by muscular exertions, are so much strengthened to-day, that the recurrence of those effects is now very frequent. This day she has first betrayed an apprehension of want of support, or, as it is commonly called, fear of falling. Whether this be an acquisition from improved powers of observation, or the consequence of some accidental shock, I am at a loss to conjecture.

* Since the time above referred to, I have witnessed a much earlier voluntary use of the eyes in another infant. Before the end of the first hour after birth, her attention being directed to a window, she cried from displeasure at its being shut, and ceased upon its being again opened; and she not only followed my hand with her eyes, when moved before them, but altered the positions of their axes accordingly as the distance of the hand was varied.

Seventh

Seventh day. The disposition continues, to open the arms and cause rigidity in the system by universal muscular exertion, when suddenly removed in the perpendicular, or likely to fall from support at too small a surface. I think she begins to know her mother independently of smell and the habits of contact, which infants certainly exercise, and distinguish much earlier. She also knows me so far as to attend to my voice and actions much more immediately and steadily than to the nurse, and her sisters.

7th day. Distinguishes persons.

Eighth day. She employs herself very actively in pursuing the objects in the room as they pass her in the relative motion from carrying her about. She seems much more conscious of the optical change of position, than of the progressive motion she herself undergoes. I incline most to the opinion that she has no notion at all concerning it. The novelty of using the hands automatically, or by mere opening and shutting, has for some days gradually worn off, and she now uses them with somewhat more of intelligence, and less incessantly. This day she used the finger and thumb alone; but all by mere contact, not in the least in conjunction with the eyes.

8th. Looks after objects.

Use of the hands by feeling only.

She looks very directly and steadily at objects. While she was lying on the nurse's lap, with her eyes steadily fixed on my face at the distance of three feet, and her countenance expressive of pleasure, I altered my position with various degrees of suddenness and distance on the one and the other side. She immediately pursued me with her eyes, and observed me with the same apparent precision as an adult would have used.

I have not observed her squint since the fifth day. I am disposed to think she does not yet feel the necessity of focal adjustment. Variation of angular position is to her much more striking than mere approach and recess. I have not seen her vary the situation of the optical axes from actual change in the distance of the objects she attends to.

Ninth day. She certainly knows her mother and me, and regards us differently. Ideas of enjoyment, comfort, refreshment, are probably associated with the voice, actions, and figure of her mother. Ideas of mental entertainment

9th. Clear distinction and preference of persons.

tertainment or amusement with me. Her mother's presence seems to excite affection; mine, gladness. When affronted by dressing or undressing, she will cease her cries to listen to the phrases of endearment she is used to hear from me.

10th. Focal adjustment of the eye.

Tenth day. The hand was moved crosswise before the infant at three feet distance, who soon fixed her attention upon it. It was then held still, but the finger and thumb gently opened and shut, to prevent her from withdrawing her notice. Under these circumstances, the hand was gradually brought nearer to her face, and she undoubtedly adjusted the optical axes to the several distances.

This trial was several times repeated in the course of the day, with the same event. When the finger and thumb were brought within the limit of distinct vision on one occasion, the effort of squinting was so disagreeable to her that she shut her eyes, and by a slight exertion or shake of the head, resisted the habit she had been betrayed into. I did not think fit to irritate her by repeating the solicitation. Hence it appears, that she has acquired the adjustment of the eye for distance, though, perhaps, she may be very careless or indifferent about exerting it.

Tremulous motion of the chin.

Yesterday and to-day she has had, at times, a quick involuntary shivering of the chin, very common to infants, but which I do not understand. I think it occurs when her mind is intent upon articulation. It may, perhaps, arise from an indistinct voluntary muscular exertion, or, perhaps, be merely automatic.

11th. First connection of the senses of contact and sight.

Eleventh day. Though she improves daily in the use of her hands, she has no notion of connecting the senses of touch and sight, except with regard to the breast. She sucks for a time, and when satisfied, she makes alternate trials of touch with the mouth, and withdrawing to contemplate the breast by sight.

Fear from sounds.

She has a strong association of danger with sudden or strange sounds, or perhaps the sounds themselves are irritative and unpleasant.

12th. Calm contemplation of remote objects.

Twelfth day. Distant and motionless objects now engage much of her attention. She contemplates them for a time,

a time, and then changes her object, as if reflecting and comparing. But her manner differs greatly, accordingly as she is more or less thoroughly awakened or attentive. The indolent enjoyment of mere sensation is probably the state of all minds not roused by motive. She is much more calm and indifferent, and her apparent progress less rapid, than when every thing was a novelty to her. It gives her more pleasure to see me at the distance of five or six feet than at the old distance of three. She will attend to my talk, but cares much less for me than before. She views her mother with the most lively affection. Though she is more comfortable and happy in my arms than when with any other person but her mother, yet she now very much prefers her mother to me; which she did not at first.

Infants smell very acutely; but do not distinguish tastes at first.

Contrary to an assertion of Rousseau in his Emilius, the sense of smelling in this, and other infants in general, is very acute. They even suck the breast at first by an action of the nostrils, like that of a scenting dog. But the sense of taste is not origiually very acute. Medicine, food, and the milk, are then almost indifferently taken. But this infaut, at present, prefers the breast very much to water-gruel, and rejects a mixture of rhubarb, chalk, and ginger, with abhorrence.

Concluding remarks.

I shall here suspend my journal for the present, which I am inclined to suspect will be read with some surprise by those who have indolently adopted the opinion, that infants of much more advanced age than during the first fortnight possess very little sense, or moral discrimiuation. If the contrary opinion, or the truth, were adopted, it would conduce much to the general sum of the happiness of these little beings, who are exerting their industry and diligence in acquiring ideas and language to a degree which is seldom suspected. Others may, perhaps, suppose extraordinary acuteness in the parent, or uncommon ability in the child, to afford an history of its progress during so few days: but here also there would be error. The child is indeed above the middle rank for memory and intelligence, and the father is capable of observing facts and writing them down. But I have never yet met with an infant who would not willingly enter into conver-

The preceding narrative not at all extraordinary.

sation to the extent of its acquisitions and ability, and to the great surprise and diversion of the parents, who never imagined any such thing. Persuasion; promises; lies; meum and tuum; individual character, as to benevolence or its reverse from physiognomy, and as to morals from the behaviour; preference of other infants from similarity of action; jests; mockery; theatrical simulation in the management of dolls, puppets, &c. &c.—these, and a prodigious number of other compound associations, occupy the minds of infants long before the expiration of the first six months of their existence.

I remain, Sir,

Your constant reader,

R. B.

VII.

Repetition of the Experiment in which Acids and Alcali are produced in pure Water by Galvanism, (no Animal or Vegetable Matter, nor Oxidable Metal being present) by Mr. CHARLES SYLVESTER.

Mr. NICHOLSON.

SIR,

Experiment of the production of alcali, &c. from water by galvanism is referred to.

IN your number for June last, I communicated some experiments tending to prove the production of the muriatic acid and an alcali, from pure water, by means of galvanism. I had not then an opportunity of giving you the whole of the experiments I had made in this dubious research. The first way in which I made the experiment was by bringing two wires of platina, or gold, from the two ends of the trough into a portion of pure water in one vessel, but I never produced the smallest quantity of muriatic acid. I am not surprised at Mr. Wilkinson's finding the same result.

Muriatic acid cannot be produced in a single vessel.

Mr. Peel and Mr. Pachiani made their experiments in the same way, and it is my opinion they never produced the muriatic acid, nor an alcali.

But the experiment succeeds with separated

If each of the platina wires be made to terminate in a separate portion of pure water, and these portions of water be afterwards connected together by an animal substance,

tance, or any substance containing moisture, the experiment will succeed. In my first attempt by this method, I separated the two portions of water by means of a bit of very thin bladder, which was tied tight upon the end of a glass tube, the tube was filled with pure water, which the bladder did not suffer to escape. Another portion of water was put into a wine glass, the tube was then immersed into the water of the wine glass, with the bladder downwards. The two portions being now separated by the bladder, the positive wire was brought into the tube, and the negative one into the wine glass. If with a moderate sized apparatus the process be continued for even half an hour, a perceptable quantity of muriatic acid will be found in the water of the tube, and a portion of fixed alcali (I believe soda) in the wine glass. I repeated this experiment more than thirty times with the same piece of bladder, and had always the same result. Though I was at the time perfectly satisfied as to the production of an acid and an alcali, I still thought, consistent with your query in the margin, that the bladder might have contributed to the production of the acid. But from whence came the fixed alcali? a very ingenious chemist who has made the same experiment, says it comes from the glass vessel; the thought appears very plausible, but I believe it is not a fact.

vessels connected by animal substance.

If the acid be afforded by the animal substance, whence comes the alcali

From the glass?

Consistently with my promise to you, I have made the experiment without the aid of either animal or vegetable substance to separate the two portions of water, and instead of a wine glass, I made use of a platina cup.

Experiment repeated without any organized matter.

In order completely to exclude glass, I got a tube made of tobacco-pipe clay, being closed at the bottom, and capable of holding water. The tube was burned in the same furnace with a quantity of tobacco-pipes and was extremely hard. In the first place I suffered the pores of the tube to be saturated with distilled water; I then filled it with the same and fixed it in a platina cup, also containing pure distilled water. The platina cup was placed upon the copper end of the trough, a platina wire reaching to the bottom of the tube was then connected with the zinc end. In a little time bubbles were copiously given out from the bottom of the cup, and also from the

With a tube of baked clay and a cup of platina.

The experiment succeeded and muriatic acid and an alcali were produced.

wire in the tube. In ten minutes from the commencement, the water in the cup was so changed as to be capable of turning yellow turmeric paper, brown; the water in the tube betrayed at the same time the presence of an acid. After the process had gone on for about four hours, I tried the acid part with sulphat of silver, and had a decided precipitate of muriat of silver.

The alcali was a fixed alcali.

The liquor in the cup containing the alcali, I then evaporated to dryness, and afterwards heated the cup nearly red hot; a residuum remained in the bottom, to which I added fresh distilled water. I found the alcali still present; a proof that it is a fixed alcali.

Inferences that the nitric and muriatic acids and all the alcalis are oxides of hydrogen.

In this experiment the alcali was much more abundant than the acid. I have seen in other experiments the acid most predominant; sometimes the nitric acid is formed, and little or none of the muriatic. From the above facts it would appear that the nitric and muriatic acids, as well as the three alcalis are oxydes of hydrogen. At the positive wire, a portion of water is decomposed, the nascent oxigen combines with a portion of water, and forms the nitric or the muriatic acid. The hydrogen is then carried by the electricity, through the bladder or other substance to the opposite wire, where the nascent hydrogen combines with a portion of water to form the alcali, so that the muriatic acid would be water, plus oxigen, and the alcali would be water, plus hydrogen. Hoping that some of your ingenious correspondents will soon confirm these facts,

I remain, Dear Sir,

Your most obedient servant,

CHARLES SYLVESTER.

26, Noble Street, Cheapside,
August 19, 1806.

VIII.

Experiments and Observations on the Adhesion of the Particles of Water to each other. By Benjamin, Count *of* Rumford, *F. R. S. &c. Communicated by the Author to the National Institution of France, and transmitted by him to the Editor.*

Small bodies of considerable

WE often see small bodies of a specific gravity, much exceeding that of water, float upon the surface of that

 exceeding

fluid. Such for example, are very small grains of sand, fine filings of the metals, and even small sewing needles.

specific gravity may float on water.

So extraordinary a phenomenon has not failed to excite the attention of philosophers. It formed a subject of discussion at the last sitting of the Class, and as this remarkable fact is intimately connected with a subject of research upon which I have been long employed, I shall here give an account of some experiments I have made to elucidate the same, and have afforded results of considerable interest.

Suspecting that the presence of air adhering to these small floating bodies, which is generally considered as the cause of their suspension, is not indispensably necessary for the success of the experiment, I made the following:

This effect attributed to air.

EXPERIMENT I.

Having half filled with water, a wine glass one inch and a half diameter at its edge, I poured on the surface of the water a stratum of sulphuric ether, one inch and a half in thickness; and when the whole was perfectly still, I took a very small sewing needle with a pair of pincers, which I introduced below the ether, where holding it horizontally at a small distance from the surface of the water, I let it fall. The needle descended to the water and there floated on its surface.

Ether was poured upon water. Small bodies descended through the ether and floated on the water.

EXPERIMENT II.

Having melted some tin I poured it into a spherical wooden box, and shaking it strongly, the metal in cooling was reduced to powder which was then sifted.

On examining this powder with a magnifier, it appeared composed of small spherules of different sizes; but these spherules were too small to be distinguished by the naked eye.

Granulated tin descended through the surface of the ether and rested on that of the water.

I took up on the point of a spatula a very small quantity of this metallic powder, and poured it gently from the height of a quarter of an inch on the surface of the ether which rested upon the water in the glass.

The powder descended wholly through the ether, and when it arrived at the surface of the water, it remained floating.

EXPERIMENT

EXPERIMENT III.

The same experiment with small globules of mercury.

Having poured a large drop of mercury into a china plate, I broke it into a great number of small spherules.

In order to take up and convey these small spherules one by one, I made a small tool or shovel out of a piece of brass wire, five inches long and about one twentieth of an inch in diameter, bended to a right angle at one of its extremities. This bended part was about a quarter of an inch long, and was hammered flat, sharpened, and made a little concave.

By means of this tool I took up a small spherule of mercury, about one sixtieth of an inch diameter, which I carefully conveyed into the stratum of ether to the distance of about one twentieth of an inch from the surface of the water beneath; and there, by a little inclination of the instrument, I caused the spherule of mercury to roll gently on the surface of the water.

The spherule descended to that surface and there remained floating.

The floating heavy body formed a kind of bag or cavity in the surface of the water.

When the eye was placed lower than the surface of the water, and the spherule was observed by looking upwards through the glass; it appeared suspended in a kind of bag, a little below the level of the surface.

Having placed a second spherule of mercury on the surface of the water, it immediately moved towards the former, and approaching it with an accelerated motion, fell down into the same cavity, which then became longer; but the two spherules did not unite.

Having placed a third spherule on the surface of the water, it joined the two others, but the weight of these three spherules together being too great to be supported by the kind of pellicle which is formed at the surface of the water, the bag was broken, and the spherules descended through the water to the bottom of the vessel.

—which breaks when the body is too heavy,

When the experiment was made with a spherule of mercury, a little larger, namely about the fortieth or fiftieth of an inch, it never failed to break the pellicle of the water, and to descend through that liquid to the bottom of the glass. But when the viscidity of the water was increased by dissolving a small quantity of gum arabic in it, still larger spherules of mercury were supported at the surface of the liquid.

A spherule

A spherule of mercury of a proper size to be supported by water, at its surface, if placed gently there, would not fail to make its way through the pellicle of the water, if let fall from too great an height.

or let fall from too great a height.

All the preceding experiments were repeated with a stratum of essential oil of turpentine, and afterwards with one of oil of olives, placed on the water contained in the glass instead of the ether, and the results were in all respects similar. I thought however that the spherules of mercury which were suspended upon the water were rather larger when the surface of the water was covered with oil than with ether; and in the experiments made with the powder of tin, poured on the oil, the finest parts of the powder, in very small quantity, floated on the surface of the oil.

The experiments answer equally well when oil is poured on the water instead of ether.

EXPERIMENT IV.

Having found means to place a stratum of alcohol on the water contained in the glass, so that the two liquids appeared as distinct from each other, as when the upper stratum was oil, I poured from a very small height a small quantity of the very fine powder of tin upon the alcohol.

With alcohol the bodies do not float.

This powder totally descended through the alcohol, and the water, without giving the smallest indication of its having been subjected to any resistance at the surface of the latter fluid.

Though this last surface appeared very distinctly to the eye, yet judging from the manner in which the metallic powder descended to the bottom of the glass, I am disposed to think that it had no existence; and in fact it is probable that it was destroyed by the chemical action of the alcohol in contact with the water.

In order to examine more accurately the kind of film which is formed at the surface of the water, I made the following experiment:

EXPERIMENT V.

In a cylindrical glass with a solid foot, the diameter of which was fourteen lines or about an inch and a half English, and ten inches in height, I poured very limpid water to the height of nine inches, and on the water I placed a stratum of ether, three lines or twelfths of an inch

When the first experiment is made with a glass of small diameter, the effects shew that a kind of

film exists at the surface of the water.

inch in thickness. I then placed on the surface of the water, a number of small solid bodies which remained suspended, such as a small spherule of mercury, some pieces of extremely fine silver wire, two or three lines in length, and a little of the powder of tin. When the whole was perfectly tranquil, I took the glass in both hands, and carefully raising it, I turned it three or four times round its axis with considerable rapidity, keeping it in a vertical position. All the small bodies suspended at the surface of the water, turned round along with the glass and stopped when it was stopped: but the liquid water below the surface did not, at first, begin to turn along with the glass, and its motion of rotation did not cease all at once upon stopping that of the vessel. In fact, all the appearances showed that there was a real pellicle at the surface of the water, and that this pellicle was strongly attached to the sides of the glass so as to move along with it.

—which being touched all the floating bodies tremble.

Upon examining with a good magnifier through the stratum of ether, the small bodes which were supported at the surface of the water, the existence of this pellicle could no longer be doubted; more particularly when it was touched with the point of a needle. For in this case all the small bodies were observed to tremble at the same time.

Having left this small apparatus at repose in a quiet chamber until the stratum of ether was entirely evaporated, I examined it again with a magnifier. The surface of the water was precisely in the same state; the small solid bodies were still there, in the same situation, and at the same distances from each other.

With a larger vessel the film near its centre is less affected by its adhesion to the sides.

When this experiment was made with a cylindrical glass of much larger diameter, the effects of the adhesion of the pellicle of the water to the sides of the vessel, were much less sensible, with regard to those parts of the same which were situated near the axis. It was difficult to prevent the small bodies which floated on the surface of the water, from uniting, and when united they often formed masses too heavy to continue to be supported; and having broke the pellicle of the water, they fell to the bottom of the vessel.

[The Conclusion in our next.]

IX.

Experiments on the Culture of Carrots. By Mr. W. WALLIS MASON.*

SIR,

THE purport of this communication is to explain with a degree of accuracy, the general, and as far as possible the best method to cultivate carrots. I shall therefore endeavour to set aside those prejudices, which frequently occur in every branch of agriculture; while I give a brief statement of particulars, which experience, assisted by numerous comparisons, has induced me to consider as best for adoption for rearing the plants, as well as most judicious in the application of the vegetables when cultivated. In Suffolk, the culture of this highly valuable root has been carried on for ages; but of late years it has very much increased, and furnishes the best criterion of its worth; various have been the attempts to extend the benefit more generally throughout the kingdom, but with little success; imaginary difficulties arising in the minds of cultivators, which I hope to obviate by a more minute detail, the observance of which will enable any practical farmer, on a proper soil, to raise a crop, which will at once be productive of great private advantage and public utility. On most farms it will be found, that a considerable proportion of the produce from the best land (the meadow and upland pasture) is consumed by the laborious cattle, and the lean and rearing stock during the winter months. The carrot system may be carried on, on inferior arable lands, and the produce, by judicious application, will be found to excel

The carrot is much cultivated in Suffolk.

* From the transactions of the Society of Arts, Vol. XXIII, just published. The Society awarded the Silver Medal for this Communication.

It requires only inferior lands. far beyond general expectation that of the grazing land, which will in consequence be appropriated to great national advantage, by furnishing an additional supply of animal food, of wool, and the produce of the dairy.

Preferable Soil. A red loamy sand is at all times to be preferred, as free from stones as possible; but very large crops may be grown on any land, which is not of a too tenacious or binding quality, with sufficient depth of soil.

To be ploughed deep. In order to increase the luxuriance of the root, it is necessary to remove the soil to the depth of 14 inches: this is easily accomplished, by first ploughing the furrow seven inches deep in the usual manner, then follow with the second plough in the same furrow, which, by the assistance of an additional horse, brings up the soil from the depth required. The first plough continues to turn the fresh furrow to the bottom of the double furrow, and being followed by the double furrow, as in the first instance, the soil becomes completely mixed and ready for the reception of the seed.

The first furrow is seven inches deep, and is removed into the ....
second furrow, of fourteen inches deep; this in rotation becomes the first stratum.

The lands, or stitches cannot be too wide, from 18 to 25 yards.

It is necessary to observe, the land at all times on which this crop is intended to be produced, should be in a perfectly clean state; a barley stubble which succeeded a fallow, &c. Yet few crops turn out more productive than those cultivated on clover, or lays of artificial grasses; ploughing the same as on a barley stubble.

Sowing to be immediately after ploughing. A rule which in most instances holds good, must not here be neglected, that of getting in the seed directly after the ploughs; a neglect of this would be attended with the worst consequences; on stale land the weeds would, in a short time, completely get the better of the young plants, and thereby occasion a great deficiency in the crop.

Five

Five pounds of seed is commonly sown per acre; but as its value, comparatively speaking, is very trifling with the advantage of a good plant, I never recommend less than six pounds per acre. In a dry season there is a great benefit in steeping the seed for twenty-four hours; to prepare it for the drill, or for sowing, it should be well rubbed with the palm of the hand against the side of a tub, to destroy the small fibres and prevent their adhesion, and a proportion of fine sifted marl and saw-dust mixed with it; the proportion two-fourths marl, one-fourth saw-dust, to one-fourth of seed. Quantity of Seed per acre, &c.

Drilling is indubitably the best way to get in the seed, from six to nine inches asunder: the advantage is obvious: the carrots stand the winter much better: from the tops of the vegetables being nearly buried in the soil, the green head only is visible to the eye, and it is very rare to see the smallest part of the red carrot above the surface. An additional advantage in this mode of cultivation, is the great facility it furnishes in weeding and hoeing, which, in a district not hitherto acquainted with this useful branch of agriculture, must render it in a twofold degree desirable.

Carrots in the early state are very tender plants, and very slow in growth; I have frequently noticed a field scarcely visible to the eye, three weeks or a month after sowing, which has turned out a most abundant produce. It is frequently six weeks before they are fit to hoe; but to prescribe any rule is impossible, since the vegetation of every description of plants so much depends on the season. I shall only observe, the most proper time to commence weeding or hoeing, is soon after the plants gain the parsley leaf, or about half-inch out of the ground. Every vegetable intended to be thinned or separated by the hoe, cannot well be done too early, since from general observation it is clearly ascertained, that the smaller the plants, the greater is the number left; and as a second hoeing is absolutely necessary (if it is only to promote vegetation by loosening the surface), the plants may then be distributed as requisite. In hoeing of every description, it is always necessary to stir every part of the soil possible; in this instance it must on no account be neglected.

Experimental Instructions for the Culture of Carrots.

The season for sowing, is from the middle of March to the 12th of April. In dry weather it is best to leave the seed rolled down. The land should always be harrowed after drilling or sowing; from the nature of the plant, a pulverization of the soil is requisite. It is, however, useless to detail particulars of this sort, which must in so material a degree depend on the state of the season, in which the judgment of the practical farmer cannot easily fail; suffice it to say, the lighter, the finer, and the less binding the soil, the better vegetation must flourish.

With respect to the best method of cleansing the young crop, I have only to observe, that nine times in ten it answers better to weed by hand, than to hoe the first time; this rests on a supposition, that the crop is much encumbered by weeds; on the contrary, (which is rarely the case) supposing it perfectly clean, the hoe will answer every purpose requisite. There is great judgement to be observed in the first hoeing, particularly to leave the plants sufficiently thick, and not to bury them in the process; should this be done, your fairest prospects will at once vanish. The women and children employed to weed, should not be suffered to pull a single carrot plant; the hoe effects the purpose of setting out in a superior manner, and should within two or three days follow the weeders I have frequently seen the land so much covered with weeds, that the plant of carrots was extremely doubtful; after hand-weeding, a very good plant was seen, which would have been destroyed in great measure, had the hoe been previously used. One weeding and two hoeings are generally sufficient; by the time they are accomplished, the carrot tops generally are of sufficient growth so shade the land. The proper hoe, to be made use of should be 4 inches wide, by 1½ inch high, and always kept very sharp.

Carrots, like turnips, and other vegetables intended to be housed for winter, should not be taken up before they are full grown; they never answer better than when used from one to four weeks after they are out of the ground. They are little liable to injury in winter; the latest time for taking

ing up, is just before the fibrous roots begin to shoot in the spring; at which period the vegetable becomes less nutritive, at the same time injurious to the land. Experimental Instructions for the Culture of Carrots.

By these attentions I have invariably found the cultivation of carrots extremely beneficial to the land, and not unfrequently the value of the crop equal to the fee simple of it. The greatest produce I ever remember was eighteen loads per acre, forty heaped bushels to the load; yet I have heard of much larger crops.

Worn-out ploughed lands are renewed by the intermixture of fresh soil occasioned by the deep ploughing; and the proof is visible in many succeeding crops of corn, grasses, &c.

The same land will produce very good crops of carrots for years in succession; but in this instance manure becomes necessary. They are taken up with a narrow spade, which the labourer strikes with one hand into the ground, pressing it sideways at the same time; he draws the root with the other, throwing it to the heap, where sits his wife and children to cut off the tops: the tops are left and spread as manure to the land.

Expences of Labour.

	s.	d.
Weeding varies from 5s. to 10s per acre, average	7	6
First Hoeing	7	0
Second do.	5	6
Taking up per load, and topping	1	2

Observing these prices, it is necessary to remark, the labourers, in dear seasons, have an allowance for flour.

To every single man one stone of flour per week, the master paying the additional price above two shillings per stone.

To a man and his wife one stone and a half per week, and half a stone per week to every additional child under twelve years old, at which time they are deemed capable of earning their own bread.

By the introduction of this judicious plan, the labourer shares the benefit of that grain which his own industry had helped to cultivate, and feels but in a small degree the oppression

Experimental Instructions for the Culture of Carrots.

pression of the times; the interest of the master and the servant becomes reciprocal, for the price of labour continues nearly at the usual standard; had it been otherwise, the farmer must have suffered when his commodities became of less value.

The annual rent of those lands on which carrots are generally grown, is from 5s. to 20s. per acre; but I have invariably found the profit by far the greatest when the best soil has been made use of :—

A good crop on land worth 5s. per acre	7 loads.
———— on land worth 10s. per acre . . .	9 do.
———— on land worth 15s. per acre . . .	11 do.
On the best land, as I before remarked . : . .	18 do.

The advantage in preferring good land is obvious, the chief expences being nearly the same as on poor soil; the additional labour consists chiefly in taking up.

Carrots are sometimes sown when the land has received but a single furrow, a sure badge of indolence. The annexed Drawing is to prove the necessity of deep ploughing, by means of the double furrow. *Fig.* 1. is the shape and comparative size of a carrot grown on a single furrow; the earth below where the soil was stirred, acting as a repellent, checks the growth of the root, and causes it to shoot laterally.

Fig. 2, is the comparative growth and shape of a carrot grown on the double furrow.* On all soils which are adapted to this branch of husbandry, the first ploughing may be done by a pair of horses abreast; the lower, or double furrow, by three horses abreast. The nearer the cattle are to the work, the greater the purchase; they labour with greater spirit in sociable pairs than in a drone-like string at length.

It is a common custom with the cultivators of carrots to raise their own seed: it requires little attention, and the

* We have not copied the Author's drawings. They shew that the carrot in the deep furrow shoots downwards to a greater distance, so as to be half as much more in size than the other. Editor.

crop

crop is seldom known to fail.—For this purpose choose such carrots as are in no respect injured by frost; and the handsomest of a middle size; trim the green top, leaving about an inch of it, and cut two inches off the extremity of the root. Plant them in double rows, a foot wide, and six inches in the row; the interval of the double rows three feet: this is requisite, as the seed does not ripen together. The path or interval serves to gather the seed, which must be done daily as the heads of seed arrive at maturity: it is frequently three weeks before the crop is cleared. Spread the heads of seeds to dry on a floor, or in dry weather on the ground; afterwards separate the seed from the stalks with a comb. The season to plant carrots for seed, is the latter end of February or the beginning of March, when the severe frosts are over.

Experimental Instructions for the Culture of Carrots.

Having explained, in as concise a manner as possible, what is necessary to be observed, to enable the practical farmer to cultivate this highly valuable root, in districts hitherto deprived of the great benefit it affords to the community, and the great profit to the cultivator, free from all theoretical and speculative opinions, I proceed to a short detail of the use and application of carrots when cultivated. On their utility for family consumption, it will not be necessary to dwell; I have therefore only to remark, since vegetables are found to be more or less nutritive in proportion to the saccharine matter they contain, but few vegetables will be found to excel them. I have known large crops of carrots sold, for the London market, at forty shillings per load, delivered at a port four miles distant from the land, which produced them a price for which a ready sale will be found in any populous town, during the winter season: for this purpose they should be assorted; all the overgrown and crooked ones reserved for home consumption, for which they will answer as well as the others; and when topped, half an inch of the green crown left on: for this purpose they are not usually washed. For home consumption I have invariably found them to answer best for the use of cart-horses; when designed for the food of other cattle, of any description, the green top must

Experimental Instructions for the Culture of Carrots

must be entirely cut off, and the carrots washed perfectly free from dirt and sand. It is necessary to house them three or four days at least before horses are fed with them; a neglect of this is sure to be attended with dangerous consequences. It is generally known that the cucumber, when left a short time in water, absorbs a proportion of it; the carrot does the same, in a less degree, yet sufficiently to produce a considerable degree of fermentation by the heat of the animal's stomach; and griping is occasioned thereby. To render them salutary, the time mentioned is sufficient for evaporation.—Washing is easily and expeditiously done, by putting a large mash-tub three parts full of carrots, then pouring cold water on them, stir them, and throw them out with four-pronged muck-forks; after which process they may be laid under cover in large heaps, as much as six or eight loads in a heap; secured from frost and rain, they will keep two or three months; it is however not right to suffer them to remain so long, in which case they shrivel even to two thirds of measure; and although they become more nutritious, from the loss of aqueous particles, it is not sufficient to compensate the deficiency. Carrots are extremely valuable when applied as food for cart-horses: when properly fed with them, they are in the greatest vigour and health; and their coats are as fine as the best-groomed coach horses, even in the depth of winter, and exposed to the inclemency of the season in a straw-yard. For home consumption, I have invariably found them to pay more, by one-third, when given to horses, than to feeding cattle. After a variety of experiments, I have found the following manner of applying them to be the best:—To each cart-horse, one heaped bushel per day, with as much cut provender as he could eat; the latter should be of the first quality. I recommend two-thirds good wheat or oat straw, and one-third clover. Wheat straw is best; oat-straw next. Barley-straw is frequently given, but never preferred, from its griping tendency. Horses cannot eat too much cut food. When returned from work, they should always be baited with it, or drink their water before carrots are given, and plenty of dry food given with the carrots: the dry nature of the one corrects

Experimental Instructions for the Culture of Carrots.

corrects the cold quality of the other. There is not any occasion to cut the carrots, but to mix them with the cut food, and feed them in the manger. Horses used to carrots will prefer them to oats, when given together. If the straw and clover are not of the first quality, oats should be given in proportion. By this method of feeding, there is a saving of at least two thirds of the hay usually consumed; corn is dispensed with; and horses will be in a better condition than when fed with hay and corn only, supposing each horse is allowed with hay half a a peck of oats per day.

Great care must be taken never to give carrots when horses come to the stable heated by work.

Carrots are not proper food for riding-horses; nimble exercise causes them to be laxative; and as they will sometimes produce griping, I shall insert a prescription which has been proved by long experience, together with the treatment to be pursued in such cases.

Oil of Turpentine	1 oz.
Castile Soap	1 oz.
Flour of Mustard . . . . . : . .	$\frac{1}{2}$ oz.

On the first symptom this mixture should be given, and it will not fail to remove the complaint. The Castile soap to be cut fine, and dissolved in a quart of boiling water, the mustard added; the oil of turpentine the last thing; it should be given more than milk warm; if the animal suffers much pain, add half an ounce of liquid laudanum. On the first appearance of the disease, the horse should be well coated, and constantly rubbed with hard twisted wisps of straw, and kept as warm as possible; should the disease increase, and the body swell much, a gallon of blood should be taken, to check the inflammation, and give time for the medicine to operate. If the symptoms increase, repeat the dose, omitting the liquid laudanum. Clysters and raking afford much relief when the symptoms first appear, and frequently remove the complaint.

Feeding cattle improve more on carrots, than when fed with potatoes or turnips; they are excellent food for ewes

 at

Experimental Instructions for the Culture of Carrots.

at lambing time, and should be cut, or they are subject to break their mouths.

Store pigs may be fattened on carrots only, and large hogs feed remarkably well, when fed with half corn and half carrots.

Heifers in calf, which require good keep and calve early, thrive better on carrots and good oat straw, than on hay only—one bushel of carrots per day—care must be taken not to give them too early or too many; in which case, the calves are liable to overgrow.

Weaned calves thrive well on this food; a peck per day is quite sufficient, more would increase the body too much.

Milking cows give more milk on carrots and straw, than on hay only In all these instances their superiority is more, comparatively speaking, than the difference of a carrot crop rated at one guinea per load of forty bushels, to the value of turnips on the same soil; rating them as a produce for home consumption.

One heaped bushel of carrots, therefore, is equal to 18lbs. of hay. Admitting each cart-horse to consume this quantity of hay for 120 days, it amounts to 2,160lbs. the average produce of one acre of good pasture land.

The same animal, if fed on carrots, with the addition of the cut-straw provender, which is a substitute for corn, and adds solidity to the carrots, will require only 120 bushels of carrots, or three loads; not half the produce of an acre of arable land worth 5s. per acre.

To this must be added the great superiority in point of condition, which the cattle evince. The latter method of feeding with carrots and cut provender, is fully equal to 18lbs. of hay and half a peck of oats to each horse.

My object in presenting the above remarks, for the consideration of the Society for the Encouragement of Arts, &c. &c. is the hope of extending a most valuable branch of Agriculture (which has long stood the test of experience) more generally throughout the kingdom; and is respectfully submitted to them by their obedient servant,

W. Wallis Mason.

Goodrest Lodge, near Warwick.
Jan. 31, 1805.
To Charles Taylor, Esq.

X.

Extracts from a Collection of Papers on the Subject of Athletic Exercises. By Sir John SINCLAIR, Bart. M.P.*

This Pamphlet consisting of 100 *pages in octavo, contains a re-print of the Observations which were before circulated by Sir John Sinclair, which were inserted in our thirteenth volume, p.* 309 *to p.* 317, *with the answers which were produced by that interesting enquiry. For the present I shall confine myself to a Letter from Dr. A. P. Buchan, on the manner of treating the Ancient Athletics.*

To Sir John Sinclair, Bart, M.P.

SIR,

My attention having been greatly excited by the very ingenious observations and queries circulated by you, concerning the method of producing, what, perhaps, with propriety, may be denominated the athletic temperament; as well as by the interesting facts contained in the communications of your various correspondents, it occurred to me, that a comparison of the modern art of training, with that practised by the antients, who certainly paid no small attention to the means of augmenting corporeal vigour and activity, would tend to throw some farther light on this curious subject. **Advantages of comparing the ancient and modern methods of training.**

Among the antient inhabitants of Greece much pains were bestowed in improving the strength and activity of the human body, by due cultivation. **The ancient Grecians.**

* Printed in London, 1806, and gratuitously circulated by the Author. I would take the liberty to suggest to the honorable Baronet, as well as to all other public spirited writers in like circumstances, that for the purpose of easy and extensive distribution, every book ought to be sold for a price at some known place, and announced once or twice in the public papers. This effectual means of placing a work in the hands of the public by the medium of booksellers, is obviously no impediment to the gratuitous distribution, which may at the same time be carried to any extent desired.

The

Gymnastic exercises.

The gymnastic exercises, so termed from their being generally performed in a state of nudity, constituted among that people an important part of liberal education, and were regularly taught by masters in schools instituted for that express purpose. Pupils were exercised in the foot race, and in the art of leaping, and of throwing the *discus* or quoit, and the javelin. These were considered as the slighter species of exercise. The more serious consisted of the art of wrestling and of boxing The combination of these two was termed *pancratium*, and seems to have been nearly equivalent to the modern English practice of boxing. When it is considered that the man who obtained a prize at the Olympic, the Pythian, or any of the public games, where candidates resorted from all the different states of Greece to contend in these exercises, not only acquired a distinction highly gratifying to himself, but which reflected honor on his family, and even on his country; it may be fairly inferred that every attention was paid to the previous education of the individuals, destined to excel in these exertions of muscular strength. Of the particular diet, and kinds of exercise in use among the Greeks previous to the solemn contest at the public games, I have not been fortunate enough to find any detailed account. Pausanias mentions that ten months previous to the solemn combat, the candidates took an oath in the temple of Jupiter, faithfully to comply with all the antient laws and usages of the champions, and from that time till the period of the solemnity they were daily and diligently exercised in whatever was requisite to produce excellence in the profession to which they had devoted themselves. A proof that the means they employed were admirably calculated to develope and improve all the corporeal powers of the human animal, is afforded by the statues of antiquity. The superiority of the Grecian sculpture, which the world has ever since attempted in vain to rival, was doubtless in great measure owing to the frequent opportunities the artists of those times enjoyed of beholding the human body brought to the highest pitch of perfection, which constant exercise in the open air, combined with appropriate regimen under a genial climate, had a natural tendency to produce, To

Foot Races.

Pancratium or Boxing.

The Candidates were previously educated, exercised and trained

To the individuals who excelled in some particular kinds of exercise, we learn from Pliny, that a statue was decreed as the appropriate reward; so that many of those figures which still remain, that of the *Discobolus*, for example, are in fact, individual portraits; and might almost without a hyperbole, be called living examples of the perfection which the human form is capable of attaining. The ancient statues were portraits of the improved human figure.

The Romans seem to have derived their inclination for public combats, as they did many other of their arts, from the Greeks. But to engage personally in these contests, appears to have been considered as incompatible with the stern dignity and decorum of the republican character. They therefore hired persons to contend with each other for their amusement. From the victor receiving a prize or reward, they were termed *Athletæ*. But the warlike genius of the Roman people soon led them to require exhibitions of a more sanguinary nature. These were performed by the gladiators, who at first consisted of captives taken in war, who were compelled to fight with each other for the amusement of the populace. Afterwards persons voluntarily embraced that mode of obtaining a livelihood, and hired themselves for money to such as chose to court popularity by treating the public with an exhibition of this kind, of which they had become extremely fond. The gladiators fought with swords and other weapons, and their combats became mortal at the will of the spectators. Athletæ of the Romans. Gladiators.

Notwithstanding the degradation of the exercises of the *palæstra* among the Romans from the rank of a liberal art, a certain degree of bodily strength and activity was indispensibly requisite to those by whom they continued to be practised. To acquire this it was requisite to comply with certain rules of regimen and exercise, concerning the nature of which a considerable share of sufficiently accurate information may still be gleaned. Regimen and exercise.

Horace acquaints us with the kinds of exercise and of privations requisite to fit a person for contending for the prize, even in the least violent of the gymnastic exercises.

> " Qui studet optatam cursu contingere metam,
> Multa tulit, fecitque puer, sudavit et alsit,
> Abstinuit venere et Baccho."

Epictetus

Training for the Olympic games as described by Epictetus.

Epictetus in alluding to the olympic games gives a somewhat more detailed account of the previous training the candidates were obliged to undergo. "I would conquer at the olympic games," he supposes his pupil to say, and then goes on to tell him; But then consider what precedes and follows, and then if it be for your advantage engage in the affair. You must conform to rules; submit to a diet; refrain from dainties; exercise your body whether you chuse it or not, at a stated hour, in heat or cold; you must drink no cold water; nor sometimes even wine. In a word, you must give yourself up to your master as to a physician. Then, in a combat you may be thrown into a ditch, dislocate your arm, turn your ankle, swallow abundence of dust; and after all lose the victory.*"

Galen was a Gymnasiach.

Galen, the celebrated physician, was himself addicted to the exercises of the *palæstra* in his youth, and has left a detailed account of the pain he suffered in the reduction of his shoulder, which had been dislocated in a wrestling match. He afterwards became a gymnasiarch, or superintendant of a company of gladiators, and many remarks on their diet, exercises, health, and habits are to be found in his writings.

Diet of the Athletæ mostly vegetable.

The diet of the *Athletæ*, in the more early ages consisted of dried figs, new cheese, and boiled grain. The antients appear to have derived a favourable opinion of the nutritious properties of figs, from observing that the persons who were appointed to guard the fig-gardens and vineyards, when the fruit was nearly ripe, and who fed upon hardly any thing else for a month or six weeks, during that period became remarkably fat. Geese were also fed on figs, in order to produce those enlarged livers which constituted a favourite delicacy of the Roman epicures. The fat and sleek appearance which the negroes, and indeed all the domestic animals in the West Indies, acquire during the season of boiling the sugar, notwithstanding the increased labour they undergo at that period, furnishes another proof of the nutritious properties of saccharine matter. It is a fact,

* Carter's Epictetus, Book iii. chap. 15.

perhaps

perhaps not very generally known, that, though a dilute solution of sugar very frequently disorders the stomach, by running into the acetous fermentation, eaten in a dry or solid form sugar hardly ever disagrees.

The governor of a gymnasium, named Pythagoras, is said to have been the first who introduced the use of animal food as part of the athletic regimen, in consequence of having observed that it produced firmer flesh, and gave more real muscular strength. Of meat, the antient *Athletæ* were restricted to the use of pork. Galen asserts that pork contains more real nutriment than the flesh of any other animal which is used as food by man; this fact, he adds, is decidedly proved by the example of the *Athletæ*, who, if they lived but for one day on any other species of food, found their vigour manifestly impaired the next. The practice of the antients differs in this respect from that of the modern trainers, who seem universally to prefer the use of beef and mutton. Perhaps these animals were not brought to such perfection, as the food of man, in antient, as they have been in modern times. The antients occasionally ate goat's flesh, which was reckoned highly nutritious, but it is said to have imparted a most fœtid and disagreeable odour to the bodies of those who used it. The preparation of meat by roasting, or broiling, was universally preferred to boiling, in which process they conceived a great part of the nutritive juices of the meat were lost in the water. Bread made of the whole flour, and unfermented *(panis azymus)* was preferred to that prepared with leaven. I have myself heard a seafaring man observe that he was always sensible of a diminution of muscular strength when he left off the use of biscuit, and ate common bread. For breakfast they took a little dry bread; but after the exercises of the day were over, they always eat to satiety, and were sometimes even forced to gorge themselves with food. Milo of Crotona is said to have consumed fifty pounds of solid food in one day. Their drink was water or some species of thick sweet wine. But they were allowed a very small quantity of fluid. This dry diet seems to have constituted an essential and important part of their regimen.

But latterly animal.

Pork preferred by the antients.

Roasted.

Bread not fermented.

The food in great quantity, but drink sparing.

They

Exercises, anointing, &c.

They were regularly exercised for many hours, daily, in every variety of muscular effort. Before engaging in the combat of the pancratium, or wrestling and boxing, the skin was anointed either with oil, or with a mixture of oil and wax, termed ceroma. This was supposed to prevent too great a loss by perspiration, as well as to supple the limbs, to grapple a man whose skin was covered with an unctuous matter of this kind was impossible; they therefore rubbed thesmelves with the dust that covered the *palæstra.* When people of rank engaged in these contests, they made use of odoriferous unguents, and rubbed themselves with a peculiar kind of pulverable earth brought from a certain cavern near Puteoli, or what was reckoned still preferable, with a kind of dust named *haphè*, which was imported from Egypt.

Refreshment by bathing, rubbing, anointing, food, &c.

When their exercises were finished they had recourse to their *apotherapia*, or methods of refreshment. They were immersed in a tepid bath, where the perspiration and sordes were carefully removed from the surface of the body by the use of the strygil. The skin was then diligently rubbed dry, and again anointed with oil. If thirsty they were permitted to drink a small quantity of warm water*. They then took their principal repast, after which they never used any exercise. They occasionally also went into the cold bath in the morning. They were permitted to sleep as many hours as they chose; and great increase of vigour, as well as of bulk was supposed to be derived from long continued and sound repose.

* Nothing can afford a ſtronger proof of the attention paid by the antients to the effects of exercife, than the prohibition of cold drink to perfons who had been thus fatigued. When heated and exhauſted by violent muſcular exertion, it is not only much more ſafe, but even more refreſhing, to take ſome warm fluid, as tea, into the ſtomach, than to drink any cold liquor. Immediate death has not ſeldom been the conſequence of drinking a glaſs of cold water or beer, after having been heated and fatigued by dancing, or any other violent exercife To thoſe who may inadvertently be guilty of ſuch imprudence, it may be well to know, that to ſwallow immediately a glaſs of brandy, or a tea-ſpoonful of laudanum, is the beſt means of counteracting its baneful conſequences.

In

In order to empty the stomach previously to entering on this particular regimen, the ancients appear to have preferred the use of emetics to that of purgatives. Vomiting was produced by tickling the fauces with the finger, or by means of a feather, which was occasionally dipped in a solution of aloes. Stimulating glysters were occasionally administered, and one of these modes of evacuating the stomach or intestines was practised whenever the appetite appeared to flag. Emetics at commencing the course.

Sexual intercourse was strictly prohibited; and during the night plates of lead were worn on the loins, with a view to prevent venereal inclinations. No sexual intercourse.

In order to exercise their patience, and accustom them to bear pain without flinching, they were occasionally flogged on the back, with the branches of a kind of rhododendron, till the blood flowed pretty plentifully. By diminishing the quantity of the circulating fluid, this rough kind of cupping was also considered as salutary, in obviating the tendency to plethora, to which they were peculiarly liable. Flaggelation.

To be exercised in a pure salubrious air was deemed of essential importance. The principal schools of the Roman *Athletæ* were accordingly established at Capua and Ravenna, places, the air of which was reckoned the most pure and healthy of any in Italy. They carried on their exercises in the open air, in all sorts of weather, the changes of which soon ceased to affect them. Salubrious air.

You will probably agree with me in remarking a considerable degree of conformity between the ancient and the modern practice of training, in the kinds of food and drink preferred, in exercise, and in constant exposure to pure and free air; the last point I should consider as being of essential importance.

The antients appear to have paid more attention to the state of the skin, by their use of the warm bath, and of friction. And the adoption of these means would probably be found useful by our modern practitioners. Nothing is more grateful after exertion, or tends to alleviate fatigue more than the tepid bath. I should imagine immersion in warm The ancients used the bath more than the moderns.

 water

water the best mode of averting the injurious effects of a boxing match.

That this regimen and exercise would have the same effects in former times, as in the present day, cannot be doubted. The ancient *cæstus*, which consisted of leathern thongs, studded with knobs of lead or copper, and contorted round the hand, must have added greatly to the force of a blow. These straps were indeed carried up to the elbow, by which the arm was in some measure protected. I doubt, however, whether any of our modern pugilists would venture to encounter such additional means of offence. By the physicians of antiquity the athletic temperament was by no means reckoned a healthy state of the constitution. Hippocrates considered this condition of extreme bodily health as peculiarly prone to disease. Galen, who, as has been already stated, was practically acquainted with the subject, asserts that besides the various accidents to which they were necessarily exposed in the course of their exercises, and combats, the *Athletæ* were liable to rupture of blood vessels in the lungs, to apoplexy, and to lethargic complaints. To obviate the last of which, they were permitted occasionally to have intercourse with the female sex. He says they rarely preserved their vigour so as to be fit to appear in public for a longer period than five years; and he particularly mentions that they were considered as a short-lived race of men. These circumstances are perhaps chiefly to be attributed to their moral conduct. For when not under a course of discipline to fit them for the combat, they indulged themselves in every kind of drunkenness and debauchery; so that by all the authors of antiquity who mention them, their manners are reprobated as being extremely dissolute.

The athletic temparament was not considered healthy.

The athletic were not long li[ved].

But they lived dissolutely.

Although that state of extreme fulness of blood and high tension of fibre, which is calculated to enable a man to exert his full strength for a short period may not be that condition of the body most consistent with permanent health, or with duration of life, yet I think you have great merit in drawing the attention of the public to the effects of

The treatment might be beneficial if judiciously adopted.

of air, exercise, and diet on the human frame, and demonstrating by such irrefragable examples, the extraordinary alteration which these powerful agents, under due management are capable of operating on the body of man.

The antients did avail themselves of this practice to restore or preserve health.

The antients were by no means unacquainted with, or inattentive to these instruments of medicine, although modern practitioners appear to have no idea of removing disease or restoring health, but by pouring drugs into the stomach. Herodicus is said to have been the first who applied the exercises and regimen of the gymnasium to the removal of disease or the maintenance of health. Among the Romans, Asclepeiades carried this so far, that he is said by Celsus almost to have banished the use of internal remedies from his practice. He was the inventor of pensile beds, which were used to induce sleep, and of various other modes of exercise and gestation, and rose to great eminence as a physician at Rome. In his own person he afforded an excellent example of the wisdom of his rules and the propriety of his regimen. Pliny tells us that in early life, he made a public profession that he would agree to forfeit all pretensions to the name of a physician, should he ever suffer from sickness, or die but of old age; and what is more extraordinary he fulfilled his promise, for he lived upwards of a century, and at last was killed by a fall down stairs.

Effects of regimen.

As some of your queries seem intended to obtain information concerning the effects of regimen in removing certain diseased states of the constitution, I beg leave to point out a few examples which have been sanctioned by experience.

Gout cured by abstinence from fermented liquors.

Several instances have come within my own knowledge, of individuals who, after having suffered severely from repeated attacks of gout, have completely eradicated that painful distemper, by an entire abstinence from fermented and spirituous liquors of all kinds, and have by the same means recovered a much greater share of health and vigour than they could expect.

The effects of the *diæta aquea*, or living wholly on pure water, cooled by ice, in alleviating the pain of cancer, and in several cases even of its effecting a complete cure of that

L 2 painful

The diæta aquea, or living wholly upon water, of great advantage in cancer.

painful disease, which are narrated by Mr. POUTEAU*, and which have been corroborated by the experience of that respectable surgeon, Mr. J. PEARSON, have, till very lately at least, been unaccountably neglected in this country. It is a singular fact, that after two or three days the desire for solid food entirely subsided, and the stomach appeared completely satisfied when filled with the aqueous fluid, of which four or five pints were drunk dail.y The pain of the sore was soon diminished, accompanied with a favorable appearance of the discharge. It is natural to suppose that a person would submit to almost any privation that promised to alleviate the anguish of so distressing a complaint; but those familiar with the manners of the diseased, know how much more readily a sick person will swallow the most nauseous drugs, than agree, to abstain from any of their habitual indulgencies.

Modern instances of the effects of regimen. Cornaro.

The example of CORNARO is the more deserving of attention and imitation, because he adopted a peculiar regimen, in order to effect a specific purpose, in which he completely succeeded. At forty years of age, he laboured under such a complication of disorders, that his life was despaired of. By strictly adhering to a measured diet, he not only perfectly recovered his health, but prolonged his life to more than a hundred years.

Wesley.

The celebrated Wesley is another instance of a delicate constitution, by strict temperance, regular exercise, and early rising, protracting his existence to nearly ninety years.

Wood, the Miller of Billericay.

Mr. Wood, the miller of Billericay, in Essex, whose case is stated in the Transactions of the College of Physicians, London, by Sir George Baker, affords an example of the possibility of reducing, by means of diet, a degree of corpulency, such as to render life a burthen, to a moderate bulk, accompanied with the return of health and strength. The miller's diet consisted of a simple pudding, made by boiling coarse flour in water, without salt. Of this he consumed about three pounds in twenty-four hours, and took

* See Oeuvres Posthumes de M. POUTEAU, Docteur en Medicine et Chirurgien én Chef de l'Hotel-Dieu de Lyon.—Paris, 1783.

no fluid whatever, not even water. On this he lived in perfect health for many years, went through a great deal of exercise in the open air, and was able to carry five hundred pounds weight, which was more than he could lift in his youth when he ate Animal food and drank freely of Ale.

A gentleman who was fond of good living, and becoming more corpulent than he thought convenient, having heard of the salutary effects of Mr. Wood's regimen, ordered his cook to prepare the Miller's pudding, which he ate with great regularity every day after his usual dinner. However ridiculous such conduct may appear, it is not very uncommon. People should be very cautious how they make any partial change in their diet or habits of living, without adapting the rest to it. Were a person, for example, to adopt so much of athletic regimen as consists in eating to satiety of animal food twice a day, and drinking plentifully of malt-liquor, without augmenting their exercise in the same proportion, undoubtedly they would soon become diseased. Lord Bacon has left us the following excellent precept on this point.—" Beware," says he, " of sudden change in any great point of diet, and if necessity enforce it, fit the rest to it. For it is a secret both in nature and state, that it is safer to change many things than one.' Imitation without judgment.

To be continued.

SCIENTIFIC NEWS.

The Society of Goerlitz, in Saxony, has offered a prize of thirty crowns, for the solution of the following Questions.

Prize Questions by the Society of Goerlitz. 1. Freezing. 2. Ancient manners, &c.

1. IN cloudy weather, it will scarcely freeze until the thermometer of Reaumar has descended to the point of Zero, or very little above it: whence does it happen, that when the sky is clear, it freezes while the same thermometer indicates three or four degrees above the point of congelation.

2. It is required to collect and arrange from the works of

of Plautus, every thing which relates to the knowledge of men and things in his time, so as to exhibit an outline of the civilization and manners of that period.

Interior temperature of the Earth.

The question concerning the absolute or relative temperature of the interior parts of the earth, has been long agitated, though it now appears to be decided that our globe contains no internal source of heat, if we except the occasional combustions of volcanoes.

Mr. De Trebra, captain of mines at Freyburg and Professor Lampadius, have directed some experiments to this object. They placed two thermometers of Reaumur at different depths in the mines, and compared them twice a day with one exposed to the atmosphere. One of the subterraneous thermometers stood constantly at 12° and the other at 9½ Mr. De Trebra, who intends to repeat and vary these experiments, purposes likewise to make some others relative to the theory of Dr. Benzenburgh on the fall of bodies.

Galvanism and Magnetism.

Ritter's Experiments on Magnetism.

Professor Ritter has communicated to the Royal Academy of Sciences, of Munich, in one of its sittings last year a course of experiments, refering directly to the nature of magnetism.

In the years 1776 and 1777, the Academy had already proposed questions on this subject, and the scientific world was busied in considering the relations which might exist between electricity and magnetism.

General facts.

The following are the results of the experiments of Ritter, as abridged by Professor Millin in his Magasin Encyclopedique for May last.

The Magnet acts like two metals

1. Every magnet is equivalent to a pair of heterogenous metals united together; its different poles represent as it were different metals.

—and gives electricity.

2. Like them, it gives electricity; that is to say, one of the two poles, the positive electricity, and the other the negative.

3. By

3. By following the same process, a certain number of magnets, as well as a certain number of pairs of metals, afforded electricity; and in this manner the electricities afforded by the poles of different magnets, have been successfully indicated by the electrometer.

Piles of Magnets will affect the Electrometer.

4. By means of these electricities, one of these batteries of magnets, accordingly as it is more or strong, produces upon dead and living bodies, all the phœnomena which are produced by a pile of Volta of the common kind, and of the same force.

—and Galvanize bodies.

5. The experiments which prove this, shew, that in magnetised iron the south pole gives positive electricity, and the north pole negative electricity; but that on the contrary in magnetised steel, the north pole affords the positive electricity, and the south pole the negative.

The poles in iron and steel have different electricities,

6. The same inverse disposition is also observed with regard to the polar oxidability of the magnetized body in which this change is produced by magnetism. In magnetized iron the south pole is most oxidable, and the north pole least; whereas in magnetized steel the north pole is most oxidable and that of the South least.

—and contrary oxidabilities.

7. Mr. Ritter thinks, that by considering the earth as an immense magnet, these results might serve to explain various phœnomena of nature, such as physical difference between the two hemispheres, the Aurora-borealis, and the Aurora Australis. In fact, after what has been just stated, the earth considered as a magnet, may be taken as an equivalent to an immense pile of Volta of which the poles are on one side sufficiently closed by the waters of the ocean. And the action of this pile must produce, and have produced the greatest chemical changes in the materials of the earth; changes which must have differed according to the poles; and of which pile the poles at the other extremity have always such an abundance of electricity as to cause its splendor to appear by radiations in the vast spaces of the heavens.

The Earth as an immense magnet, producing electric effects—

National

National Industry.

Prizes.

Prizes offered by the Society for encouragement of National Industry, in France, at its general sitting of Jan. 29 last.

—for sizing Paper.

1. For sizing of paper, a prize of three thousand francs, is offered to him who shall indicate a less expensive and more perfect process than is at present employed in the paper manufactories of France.

—Vermillion.

2. For the fabrication of cinnabar, equal to that called Chinese vermillion, a prize of twelve hundred francs.

—Block Engraving.

3. For the encouragement of engraving in relief, or producing blocks for printing, two thousand francs.

The Society invites the concurrents, to discover mechanical or chemical means of facilitating the processes of this method of engraving, without losing sight of the requisite degree of perfection. They also direct the attention of artists towards the most useful objects, such as designs of machines, and apparatus of every kind, architecture, and nautral history, in its various branches, and ornaments for letter press, &c.

The preceding prizes will be decreed in the year 1807, the following are for the year 1808.

Brick and Tile Furnaces.

1. For the best construction of furnaces for lime, tiles, and bricks, a prize of 2400 francs, and two other prizes of 500; to those who shall have approached the nearest in succession to the object of this program, of 300 francs.

The society take notice, and their remark is probably applicable to the processes made use of in this country, that great savings of fuel, and other advantages have been derived from scientific improvements in the furnaces made use of in many of the arts; but that in the fabrications which form the subject of the present question, no such improvement has taken place; they therefore offer the before mentioned prizes to those who shall establish works in France in which these articles shall be made with the least expence of fuel

Horse beans.

2. For the culture of horse beans, on a large scale, in a district where they are not at present used, 600 francs.

Jones, Printer, Chapel-street, Soho.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

OCTOBER, 1806.

ARTICLE I.

Letter from RICHARD LOVELL EDGWORTH, ESQ. *containing a Description of an Odometer for a Carriage, and some Remarks on the Patent Boring Machine of Mr.* RYAN.

To Mr. NICHOLSON.

Edgeworthstown, Ireland, Aug. 29, 1806.

SIR,

I SEND you a drawing of an Odometer for a carriage, which I request you will insert in your Journal when it is entirely convenient to you.

Description of an Odometer, or instrument for measuring the ground passed over by a carriage.

This Odometer is more simple than any which I have seen, is less liable to be out of order, and may be easily attached to the axletree bed of a postchaise, gig, or any other carriage.

One turn and a half of a screw is formed round the nave of one of the hinder wheels by a slip of iron three quarters of an inch broad and one-eighth of an inch thick; this is wound round the nave, and fastened to it by screws passing through five or six cocks, which are turned up at right angles on the slip of iron. The helix so formed on the nave of the carriage wheel acts as a worm or screw upon the teeth of the wheel A, upon the

Description of an Odometer, or instrument for measuring the ground passed over by a carriage.

arbor of which another screw of brass B is formed, which acts upon the brass wheel C. (Fig. 1. Pl. III.) This wheel C serves also as a dial-plate, and is divided into miles, halves, quarters, and furlongs; the figures indicating the miles are nearly three quarters of an inch long, so as to be quite distinct; they are pointed out by the index D, which is placed as represented in the plate, in such a manner as to be easily seen from the carriage.

These two brass wheels are mounted by the irons E E upon a block of wood F, eight inches long, two inches thick; and five inches broad. This block may be screwed upon the axletree-bed by two strong square-headed wood-screws. If the carriage permits, this block should be fixed obliquely on the axletree-bed, so that the dial-plate may be raised up toward the eye of the person looking out from the carriage.

H is a ratchet wheel attached to the arbor of the wheel A, which, by means of the click I, allows the wheel to be set with a key or handle fitted to the squared end of the arbor at K. L is a long spring screwed on the block; it presses on the wheel A, to prevent it from shaking by the motion of the carriage. A small triangular spring is put under the middle of the dial-plate wheel for the same purpose.

If the wheel of the carriage is exactly five feet three inches in circumference, the brass toothed wheel which it turns should have twenty teeth, and that which serves as a dial-plate should have eighty; it will then count five miles. If the carriage wheel is either larger or smaller, a mile should be carefully measured on a smooth road, and the number of turns which the carriage wheel makes in going this mile may easily be counted by tying a piece of fine packthread to one of the spokes and letting the wheel, as it moves slowly forward, wind up the packthread on its nave. When the wheel has proceeded a half or a quarter of a mile, unwind the string and count the number of turns which it has made.

By the addition of another wheel of eighty-one teeth placed under the dial-plate wheel and moved by the screw C, with a proper hand fitted to it, and proper figures on the dial-plate, this machine would count four hundred miles.

Boring

Boring Machine.

I take this opportunity of mentioning a trial that I lately made of Mr. Ryan's patent Boring Machine, for exploring the strata of mineral countries. This machine acts like the surgical *trepan*, and cuts a circular hole, leaving a *core* in the middle which is drawn up from time to time by a pair of self-closing tongs.

Boring machine of Mr. Ryan. It cuts out a circular piece, acting like the trepan.

I found that this gentleman's machine, from his want of knowledge how to push his own invention, had not obtained due credit; I therefore invited him to try his machine at my house, that I might witness the result of the experiment.

Two men, relieved from time to time, cut a truly circular hole, five inches and a quarter in diameter, through a block of hard limestone, leaving a *core* a little taper of four inches and a half diameter and six and three quarters in length, which core is now in my possession. It is as true and as smooth as if turned and polished in a lathe, and the under surface shews exactly the fracture by which it was detached from the block at bottom.

Five inch hole cut through lime stone.

By this contrivance mines may be ventilated at small expense, the specimens of strata that are bored through may be brought up whole and unmixed, no deception can take place; and not only the dip, but the fracture, lap, and accidents to which each stratum is liable, may be determined at any depth. True vertical and horizontal sections may be previously obtained of any spot where it is proposed to sink shafts; and the subterraneous topography of a whole district may be laid down upon a map with confidence before any great expense is hazarded on mere speculation.

The pieces being brought up entire afford a very accurate knowledge of the strata.

I am, Sir,
With regard and esteem,
Your faithful humble servant,
RICH. LOVELL EDGEWORTH.

II.

Remarks on a Paper of Mr. BOSWELL *on invariable Pendulums, &c. In a Letter from* J. G.

To MR. NICHOLSON.

SIR,

Introduction to Remarks on a Paper of Mr. Boswell.

AS you do not appear to be averse to the insertion of any critical observations upon such papers of your Journal as may happen to contain any defective inferences, I am induced to transmit you the following remarks on a paper of Mr. Boswell's, relative to pendulums, published in your Number for February 1805, which I have but just seen.

Mr. B. considers the *magnitude* of bodies submitted to pyrometrical experiments as an element of great importance,

Mr. Boswell commences his paper by observing that the magnitude of the bodies submitted to pyrometrical experiments has been very generally neglected, a point which he would insinuate to be of material importance in all considerations of this nature. He then proceeds to state the obvious circumstance that bodies are more or less speedily affected by the temperature of any surrounding medium, according to the proportional number of their particles which are in contact with this medium; a circumstance which, as he observes, is dependent on the shape and mass of the given body. From hence he deduces the following inferences:

— whence he infers that a large body would be little changed by the atmosphere.

"1. That the greater the bulk of any body, the less "will be its mutability of temperature in proportion, "and, of course, the less will it alter its degree of ex-"pansion."

"2. That a large globe, in the first place, or a cylin-"der whose height was equal to its diameter, in the next "place, or, in the third place, a large cube, would have "its dimensions very little changed by the fluctuations "of atmospherical temperature."

But Mr. B. does not attend to partial expansion.

The above inferences appear to be inadmissible from a circumstance which Mr. Boswell seems to have overlooked, viz. the possibility of a partial expansion of the given body, or of the expansion of one part independently of the remainder, Mr. Boswell's inferences requiring no expansion to take place until each particle has assumed the temperature of the surrounding medium.

It

It is well known that a bar of iron may be heated red hot at one extremity without effecting any sensible change in the temperature of the other extremity: would Mr. Boswell thence infer that no expansion whatever has taken place in the bar? will he not rather admit a certain expansion to have been produced in the heated end, and a gradual decrease of this expansion to have extended toward the cold extremity?

A bar of iron may be red hot at one end and cold at the other;

— and would expand where heated.

If the preceding effect be admitted to take place in a bar of metal, it must equally be admitted in every body which is susceptible of expansion by heat: Mr. Boswell's contrivance for rendering effective the compensations proposed by Crosthwaite or Pine must consequently be nugatory, were the apparatus affixed even to the side of a mountain of granite in lieu of his octagonal pillar. The other suggestions of Mr. Boswell, in which deal, mahogany, or metal is employed, I should apprehend to be still less deserving of attention; and I am incapable of perceiving the good effect which is likely to arise from the filling the glass tube with semen lycopodii or sawdust, or from the covering it with oiled silk.

And so would Mr. Boswell's large body.

Objection to some other of his contrivances.

In his observations on a paper published in the Repertory of Arts, relative to Pine's and Crosthwaite's pendulums, (which, by the bye, appear to me equally harsh and unjust,) Mr. Boswell triumphs in his turn, with no small satisfaction, on a supposed error of the author; a triumph which the preceding remarks will not so readily authorize; it may be even doubted whether the effect would not have been greater in the *solid plank* than in the simple rod. The cock through which the pendulum spring passed in order to effect the proposed compensation, I imagine to have been affixed to the plank by screws: now, if from the imperfect conducting power of wood, with respect to calorie, we suppose the exterior strata to expand from an increase of temperature, and that the expansion of the interior strata gradually decrease, as in the afore-mentioned bar of iron, it is evident that the screws will turn, as it were, on their interior extremity, and form an angle with the axis of the plank, supposing them to have been perpendicular to the axis before the expansion took place; consequently the outer

Mr. B.'s animadversions on Mr. Pine's paper, not founded on facts.

Explanation.

A thick plank of support may be less effectual than a thinner.

outer extremity of the cock would be farther removed from any fixed point in the axis of the pendulum than if the expansion had been uniform throughout, in which case the screws would have maintained their perpendicularity. The same reasoning will not apply to the simple rod, the expansion being equal in all the concentric strata.

Mr. Boswell's errors with regard to the pyrometer have been so ably noticed by you, that it would be absurd in me to touch upon them; but his objections to the gridiron pendulum, on account of the mixed metals of which it is composed, do not appear to have been controverted: Mr. Boswell ought, however, to have been aware that an adequate allowance for adjustment is constantly made in their construction, to obviate any defects which may arise from a faulty estimation of the comparative expansions of the different parts of the instrument, as described in the account of the excellent seconds pendulum of your own construction.

Gridiron pendulums are adjusted while actually going.

I am, Sir,
Your obedient servant,
J. G.

III.

A Third Series of Experiments on an artificial Substance which possesses the principal characteristic Properties of Tannin; with some Remarks on Coal. By Charles Hatchett, Esq. F.R.S.*

(Concluded from page 31.)

VII.

Experiments, &c. on an artificial substance having the characters of tanning matter.

AFTER the tanning substance and the other products had been obtained from the resins, balsams, &c. which have been mentioned in the beginning of this Paper, the following proportions of coal remained †:

* Philos. Trans. 1806. For the former Papers see our Journal.

† The weight of the coal obtained from each of the abovementioned substances, was estimated after the complete separation of every other product, and after the moisture had been expelled by a red heat, in close vessels.

100 grains

		Coal.
100 grains of	Copal	67 grains.
———	Mastich	66
———	Balsam of Peru	64
———	Elemi	63
———	Tacamahac	62
———	Guaiacum	58
———	Gum ammoniac	58
———	Amber	56
———	Olive oil	55
———	Balsam of Tolu	54
———	Asa fœtida	51
———	Wax	50
———	Dragon's blood	48
———	Benzoin	48
———	Olibanum	44
———	Myrrh	40
———	Asphaltum	40
———	Gamboge	31
———	Elastic bitumen	31
———	Gum arabic	29
———	Liquorice	25
———	Manna	25
———	Tragacanth	22
———	Caoutchouc	12*

Experiments, &c. on an artificial substance having the characters of tanning matter.

The coal obtained from the resinous bodies by means of sulphuric acid, is in a much greater proportion, than when equal quantities of those substances are exposed to simple distillation.

For, (as I have stated in my first Paper,) 100 grains of common resin by the humid process afforded 43 of coal, which after a red heat still weighed 30 grains.

But the same quantity of resin by distillation, only yielded ½ of a grain of coal.

One hundred grains of mastich, by the first method, afforded 66 grains of coal.

* Caoutchouc and elastic bitumen were only superficially carbonized by the sulphuric acid, so that the proportion of coal as above stated, is considerably less than that, which in reality might have been obtained from them.

 One

Experiments, &c. on an artificial substance having the characters of tanning matter.

One hundred grains of the same mastich only gave $4\frac{1}{2}$ grains of coal when simply distilled.

And 100 grains of amber, when treated with sulphuric acid, yielded 56 grains of coal.

But from 100 grains of the same amber when distilled, only $3\frac{1}{2}$ grains could be obtained.

Many other examples might be adduced, but these appear to be sufficient; and I must here observe, that the case is very different in respect to the gums, for the difference between the proportions of coal obtained from them by the humid and dry ways is not very considerable, although it is always the greatest in the former process, when conducted with precaution. Moreover it is to be remarked, that in either process, variations in the quantity of coal are produced by difference of temperature, by the figure and size of the vessels, and many other circumstances.

But it is not only in the proportion, that there is so great a difference between the coal obtained from the resinous substances by the humid way or by fire, for the quality is also most commonly different; and this not only applies to the resins but also to ligneous matter.

The coal obtained by the humid process from many of the resins, was shining, hard, and occasionally iridescent. Few of the coals obtained from the same bodies by fire had any of these properties. The combustion of the former was slow in the manner of some of the mineral coals, whilst on the contrary the latter were speedily consumed like charcoal. This difference I was at first inclined to attribute to a small portion of the acid which might not have been completely separated, and I therefore purposely made some experiments which convinced me that this was not the case.

Having remarked this difference in the coals afforded by the resins, I was desirous to make some comparative experiments on wood, and for this purpose I selected oak.

1.

On 480 grains of oak sawdust I poured two ounces of sulphuric acid diluted with six ounces of water, and placed the matrass on a sand-bath, where it remained from

from the beginning of last June to the end of September. During this time, the sand-bath had very seldom been heated, but the vessel was occasionally shaken.

Experiments, &c. on an artificial substance having the characters of taning matter.

At the end of the period above mentioned, six ounces of boiling water were added, and the whole being poured upon a filter, was repeatedly washed, and was afterwards dried on a sand-bath in a heat not much exceeding 300°.

The sawdust appeared to be reduced to a granulated coal, partly pulverulent, and partly clotted; the whole weighed 210 grains.

One hundred and five grains of this coal were put into a platina crucible, and were exposed to a red heat under a muffle. At the same time, an equal quantity of charcoal made from the same oak sawdust, was placed in another vessel by the side of the former.

The charcoal was speedily consumed, and left some brownish-white ashes, which as usual, afforded alkali, with a trace of a sulphate, which was probably sulphate of potash.

On the contrary, the coal formed by the humid way, burned without flame, similar to the Kilkenny coal, and others which do not contain bitumen. It was very slowly consumed, like the mineral coals above mentioned, and left some pale red ashes, which weighed 2 grains. These ashes *did not yield* the smallest vestige of *alkali*, and the only saline substance which could be obtained, was a very small portion of sulphate of potash, which did not amount to more than one-fifth of a grain; and it is probable, that had the coal been more copiously washed, even this small portion of the neutral salt would not have been obtained.

2.

At the time when the preceding experiment was began, I also put 480 grains of the oak sawdust into another matrass, and having added four ounces of common muriatic acid, the whole was suffered to remain during the period which has been mentioned.

At the end of the four months, the remainder of the acid was for the greater part driven off by heat not exceeding 300°. The sawdust then had the appearance of a brownish-black mass, on which about a pint of boiling

 distilled

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distilled water was poured; the whole was decanted into a filter, was repeatedly washed, and was afterwards dried without heat. The sawdust then appeared, as I have observed, brownish-black, and was pulverulent. It burned with some flame, emitted still a slight vegetable odour, and was reduced to ashes much sooner than the coal formed by sulphuric acid, but not so speedily as the oak charcoal. The ashes had an ochraceous appearance, and were almost devoid of any saline substance, excepting a very slight trace of muriate of potash.

These two experiments therefore prove,

1st. That wood may by sulphuric acid be converted into a coal which in its properties is very different from charcoal, although prepared from the same sort of wood; and that the coal thus formed by the action of sulphuric acid, resembles by its mode of burning, and by not affording any alkali when reduced to ashes, those mineral coals which are devoid of bitumen.

2dly. That wood may also be converted into a sort of coal by muriatic acid, but in this case some of the vegetable characters remain, although, like the former, not any alkali can be obtained from the ashes.

§VIII.

Four different solutions have been proposed respecting that difficult problem in the natural history of minerals, *the origin and formation of coal.*

The first is, that pit-coal is an earth or stone chiefly of the argillaceous genus, penetrated and impregnated with bitumen.

But Mr. KIRWAN very justly remarks, that the insufficiency of this solution is demonstrated by Kilkenny and other coals which are devoid of bitumen, and also that the quantity of earthy or stony matter in the most bituminous coals bears no proportion to the weight of them *.

The second and most prevailing opinion is, that mineral coal is of vegetable origin, that the vegetable bodies have, subsequent to their being buried under vast strata of earth, been mineralized by some unknown process, of

* Geological Essays, p. 316.

which,

which, sulphuric acid has probably been the principal agent, and that by means of this acid, the oils of the different species of wood have been converted into bitumen, and a coaly substance has been formed.

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The third opinion is that of ARDUINO; who conceives coal to be entirely of marine formation, and to have originated from the fat and unctious matter of the numerous tribes of animals that inhabit the ocean.

And the fourth is Mr. KIRWAN's opinion, who considers coal and bitumen to have been derived from the primordial chaotic fluid*.

The limits of this Paper will not permit me to enter into the various arguments and facts which have been adduced in the support of these different opinions; but the second, or that which regards the vegetable substances as the principal origin of coal, seems by much the most probable, because it is corroborated by the greater number of geological facts, as well as by many experimental results. Most of the former have however been stated in different works, and I shall therefore only notice a few of the latter which have occurred in the course of my experiments.

The observations of Dr. CORREA DE SERRA on the wood of the submarine forest at Sutton, on the coast of Lincolnshire, together with similar accounts which have been published in the Philosophical Transactions and other works, demonstrate in the most satisfactory manner, that whether vegetables are totally or partially buried under the waves or under the earth, they are not merely by such means converted even into the most imperfect sort of coal†. Some process therefore independent

* Geological Essays, p. 327.

† In my Paper, "*On the Change of some of the proximate Principles of Vegetables into Bitumen*," I have quoted the remarks of BERGMAN, VON TROIL, and others, on the compressed state of the trunks of the trees which have been converted into surturbrand, Bovey coal, and similar substances. The same observation has been also made by Dr. CORREA DE SERRA respecting the timber of the submarine forest at Sutton; and this is the more remarkable, as the submerged vegetables at Sutton do not exhibit any appearance of carbonization.

Experiments, &c. on an artificial substance having the characters of tanning matter.

ent of these circumstances must have taken place, in order that the vegetable substances, such as ligneous matter, resin, oil, &c. should become coal and bitumen.

In a former Paper I have endeavoured to shew, that these changes are progressive, and having noticed the perfect state of the submerged wood at Sutton and other places, I next described the qualities of the different kinds of Bovey coal, which exhibit a series of gradual changes from bodies which retain the vegetable structure and texture, although imperfectly carbonized, to others in which almost the complete characters of the common mineral or pit-coal are absolutely established.

From the alder leaves in the schistus from Iceland, I obtained extractive vegetable matter, and although this was not afforded by the varieties of Bovey coal, yet these, as well as the alder leaves, and also a coal like that of Bovey, found in Sussex, at Newick Park, (an estate belonging to Sir Elijah Impey,) and also the surturbrand of Iceland, yielded some resin, which at Bovey is likewise found in distinct masses, intermixed with the strata of coal, and combined with asphaltum, in the proportion of about 41 parts of the latter with 55 of resin *.

Now, exclusive of the other vegetable characters which are so evident in many of the varieties of Bovey coal, of the Sussex coal, of surturbrand, &c. &c. the presence of resin must be regarded as a strong fact: for this substance has always been attributed to the organized bodies, particularly to those of the vegetable kingdom, and I do not know of any instance, previous to my own experiments, in which, resin had been discovered as constituting part of any of the different species and varieties of coal.

From the external vegetable characters possessed by the

Dr. Correa says, "In general the trunks, branches, and roots of "the decayed trees, were *considerably flattened;* which is a pheno- "menon observed in the surturbrand or fossil wood of Iceland, "and which Scheuchzer remarked also in the fossil wood found "in the neighbourhood of the lake of Thun, in Switzerland." Phil. Trans. 1799, p. 147.

* Observations on the Change of some of the proximate Principles of Vegetables into Bitumen. Phil. Trans. 1804, p. 405.

the Bovey coal, the Sussex coal, the surturbrand, and many others, together with the resin, (allowed to be exclusively a vegetable substance, or at least one which only appertains to the organized natural bodies,) there cannot be any doubt, that such coals have been formed from wood and other substances belonging to the vegetable kingdom.

Experiments, &c on an artificial substance having the characters of tanning matter.

But some mineralogists attempt to draw a line of separation between the coals above mentioned and the others, which therefore they call the true mineral coals.

This opinion may in some degree be refuted even from the specimens afforded by the Bovey coal-pits, where, as I have observed, a regular gradation may be seen from wood which is but very imperfectly carbonized, to the substance called stone coal, which in every respect appears to be most nearly if not absolutely similar to the common pit-coals *.

It may however be objected, that such a transition is peculiar to this and similar places, and that the pit-coal found in other situations, where nothing resembling the Bovey coal can be discovered, is in reality of a different nature.

But this objection I think may be answered by the results of those experiments on pit-coal, Cannel coal, and asphaltum, which I have related in the third section of this Paper; for when these were subjected to the action of nitric acid not too long continued, it was found, that the acid first dissolved the principal part of the carbonaceous matter, and if then the process was stopped, there remained a substance in a proportion corresponding to that of the bitumen either in the pit-coal, or principally forming the Cannel coal and asphaltum, which although not absolutely in the state of resin, was however in a state intermediate between it and the vegetable extractive matter.

Moreover I have stated, that under similar circumstances, a substance possessing in a great measure the same properties, may be obtained from the known vegetable resins by the action of nitric acid.

* Phil. Trans. 1804, p. 398.

When

Experiments, &c. on an artificial substance having the characters of tanning matter.

When therefore, these facts are added to that of the natural mixture of resin and asphaltum which is found with the Bovey coal, we to all appearance have almost positive proof that the pit-coals are of vegetable origin.

True it is indeed, that bitumen has never been formed by any artificial process hitherto devised, from the resins or other vegetable substances. I have myself attempted it in various ways without success, for although I occasionally obtained products which resembled it somewhat in odour when burned, and other properties, yet the effects of alcohol or water always proved these products not to be bitumen.

But synthesis of natural products, although required in strict chemical demonstration, is (as we have but too often occasion to know) seldom to be attained, especially when operations are performed on bodies whose component parts are liable to an infinite series of variations in their proportions, qualities, and mode of combination.

Considering therefore, that bitumen and resin afford by certain operations similar products, that resin and bitumen are found blended together by nature, and that this mixed substance accompanies a species of coal which in many parts still exhibits its vegetable origin, whilst in others it passes into pit-coal, we may with the greatest probability conclude, that bitumen is a modification of the resinous and oily parts of vegetables, produced by some process of nature, which has operated by slow and gradual means on immense masses, so that even if we were acquainted with the process, we should scarcely be able to imitate its effects, from the want of time, and deficiency in the bulk of materials.

But although bitumen cannot at present be artificially formed from the resinous and other vegetable substances by any of the known chemical processes, yet there is every reason to believe, that the agent employed by nature in the formation of coal and bitumen has been either muriatic or sulphuric acid; and when it is considered, that common salt is never found in coal mines except when in the vicinity of salt springs, whilst on the contrary, pyrites, sulphate of iron, and alum, most commonly

monly are present*; these facts, together with the sulphureous odour emitted by most of the mineral coals when burned, appear strongly to evince the agency of the latter. That this has been the case, seems also to be corroborated, by the great resemblance which (as has been previously stated) the coals formed artificially from many vegetable substances bear to the mineral coals, especially as the similarity is not confined to external characters, but extends to other properties.

Experiments, &c. on an artificial substance having the characters of tanning matter.

By the action of sulphuric acid on vegetable bodies, a much greater portion of their carbon is converted into coal than when the same are subjected to the effects of fire.

Several examples respecting the resins, have been mentioned in the seventh section of this Paper, and the result of the experiment made upon oak perfectly accords with them.

Mr. PROUST, in the course of some comparative experiments on the proportions of charcoal afforded by different kinds of wood, obtained 20 *per cent.* from green oak, and 19 *per cent.* from heart of oak †.

But by sulphuric acid, from 480 grains of oak, I obtained 210 grains, or about 45 *per cent.* of coal, which burned not like the charcoal obtained from the same wood, but like many of the mineral coals; and this was also observed in the combustion of the greater part of the coals obtained by the humid way from resinous substances.

The experiment on oak also appears to refute another objection to the vegetable origin of pit-coal, namely, the total absence of the alkalis, which on the contrary are so constantly obtained from the ligneous parts of vegetables by combustion ‡. But I have shewn, that when these bodies are carbonized in the humid way either by muriatic or by sulphuric acid, not any alkali can be obtained from the ashes of coals so formed; and this seems also to be a farther proof, that the humid way has been employed in the operations of nature to convert the above mention-

* KIRWAN's Geological Essays, p. 324.

† *Journal de Physique*, 1799, Tome 48, p. 469.

‡ KIRWAN's Geological Essays, p. 320.

ed

Experiments, &c. on an artificial substance having the characters of tanning matter.

ed substances into pit-coal; for supposing fire to have been the agent, it does not appear easy to conceive how the alkali could have been destroyed or separated*.

Every circumstance seems therefore to support the opinion of those who consider the pit-coals as having been formed in the humid way, principally from vegetable bodies, and most probably by the agency of sulphuric acid; and allowing that animal substances may also have contributed to the production of coal, yet this would not militate against the above mentioned opinion, as the effects produced upon them by that acid would in all the essential points be perfectly similar†.

An

* Some have attempted to account for the absence of alkali in the Bovey coal and common pit-coal, by supposing that the vegetable bodies (from which these have been formed) were previously deprived of alkali by simple lixiviation during their immersion in water. But in page 127 of this Paper, I have shewn that the submerged oak of Sutton, although deprived of its tannin, still retained its potash, which certainly would not have been the case if the latter like the former could have been separated from the wood by mere solution. When wood is reduced to ashes, the alkali becomes completely denuded by the destruction of the woody fibre, and consequently may be immediately taken up by water; but when wood is converted into coal in the humid way by means of an acid, then it seems to me that two effects take place; for the intimate combination of the alkali with the woody fibre becomes in a great measure destroyed by the carbonization of the latter, whilst a simultaneous action arises in the affinity between the acid and the alkali; so that if coal has been formed by such means, the alkali must have been separated from the wood, in the state of a dissolved neutral salt.

† From the nature of the experiments which have been related in this Paper, I have unavoidably been induced to notice concisely the different opinions on the formation of coal by the humid way; but I did not intend to have mentioned any of those which have been brought forward in favour of the immediate or indirect action of fire, as I only wished to express my sentiments respecting the most probable of the former opinions.

Since however this Paper was written and partly read before the Royal Society, I have been favoured by Sir JAMES HALL, with a copy of his Paper, intitled "*Account of a Series of Experiments shewing the Effects of Compression in modifying the Action of Heat*;" and I am fully of opinion that the scientific world has not for a long time received any communication of more importance, or in which more accuracy, ability, and perseverence have been displayed. The effects which Sir JAMES HALL has produced on carbonate of lime, by heat acting under compression, certainly removes a great and at

one

An inquiry into the nature and formation of coal was my first object when I discovered the artificial tanning substance, and considering the importance of the latter, it will not appear surprising, that it should immediately have engaged the principal part of my attention.

Experiments, &c. on an artificial substance having the characters of tanning matter.

In addition to the experiments which have been related in the three Papers upon this subject, I intended to have decomposed the different varieties, to have compared their gases and other products with those of the natural substance, called Tannin, and especially to have endeavoured to discover more economical methods of obtaining the artificial product; for, exclusive of speculative science, this appears to be an object of consequence, not only respecting that useful and valuable branch of manufacture, to which it immediately relates, but also as the means of preventing, or at least of diminishing, the premature destruction of timber in a country, where, on account of its population, as well as on account of its maritime position, every economy in such an article should be most rigidly observed.

But for the present, I intend to relinquish this subject to such as may consider it worthy of attention; whilst,

one time apparently insurmountable obstacle to the HUTTONIAN or PLUTONIAN theory, and if they do not solve the grand geological problem, they must even, in an insulated point of view, be allowed to have opened a new and unexplored field of research in chemistry as well as in geology.

In the 8th section of this valuable Paper, the author has given an account of some experiments made on leather, horn, and fir sawdust, from which he obtained coal which burned with flame, and which apparently resembled some of the mineral coals. In one case also, he obtained a substance, which in external characters appeared somewhat similar to the mixture of asphaltum and resin found at Bovey, to which I have given the name of Retin-asphaltum. These experiments Sir JAMES HALL intends to resume, and it is my earnest wish that he would do so; for although I am strongly inclined to believe that the mineral coals have generally, if not always, been formed by some humid process, yet it is impossible to foresee the results which may be obtained from animal and vegetable bodies subjected to the effects of heat modified by compression, as the principles of these bodies may be acted upon, and may be made to re-act on each other, under circumstances which until now have not been imagined.

Experiments, &c. on an artificial substance having the characters of tanning matter.

as I have already stated, I entertain very sanguine expectations, that eventually it will prove economically useful; and should any be inclined to pursue the inquiry, I would recommend particular attention to those processes which relate to the roasted vegetable substances, and to peat.

Almost any refuse vegetable matter, such as twigs, dead leaves, &c. will serve for the former; whilst the latter, as I have shewn, does not require to be roasted, and in many, especially the northern counties, peat is found in such abundance, that but a small proportional quantity is consumed in the only useful way to which it has hitherto been applied, namely, fuel.

Before I conclude this Paper, I shall also observe, that the experiments which have been described, must be regarded only as a mere sketch of that which may be performed, whilst the facts which have been ascertained respecting the resins, balsams, gum resins, and gums, serve to prove, that much may be expected from regular chemical examinations of these bodies. But such investigations, in order that science may truly be promoted, should be strictly regular: that is, they should not be taken up in a desultory manner, but these substances should be comparatively and systematically examined with all the accuracy which can be employed in the present state of chemical knowledge; for as this knowledge concerning the composition of organized bodies is confessedly very imperfect, I am persuaded, that like other of the sciences, chemistry will be less liable to error, when guided by comparative experiments and comparative analysis.

III.

Discussions relating to the Claims of Lavoisier as an Inventor of Chemical Theory. In a Letter from JONATHAN STOKES, *M. D.*

Chesterfield, Aug. 31, 1806.

To MR. NICHOLSON.

SIR,

It is strange that Lavoisier in his last me-

I HAVE perused with much satisfaction your remarks and those of your correspondent E. D. on the subject

ject of Lavoisier's claim to the establishment of the present theory of what may be called aërial chemistry. I have ever regarded him as its author, but the mode in which he has asserted his rights appears so reprehensible, that nothing but my entire conviction of the integrity of his amiable relict could induce me to believe that the fifth memoir translated in your Journal for Jan. p. 81, contains the whole of what he wrote on that subject. Had the editor been unknown to me, I should have said—is it possible that Lavoisier who did so much justice to the discoveries of his predecessors in his Opuscules, could have entered so minutely into the Theories of Rey, Lomery, Charras, Stahl and Morveau, and yet omit to notice the experiments of Hales and Priestley? why has the editor omitted to print the following passages which he must surely have found in the author's hand-writing? "Tous "les physiciens de son temps pensoient que le feu se fixoit se "combinoit avec les metaux, et que c'étoit cette addition "qui les reduissoit a l'état de chaux. M. Hales ne "s'est point écarté de cette opinion; mais il a de plus "avancé que l'air contribuoit a cet effet, et que c'étoit "en partie a lui qu'étoit du l'augmentation de poids des "chaux métalliques. Il fondoit cette opinion sur ce "qu'ayant soumis 1922 grains de plomb à la distillation, "il n'en avoit retiré que 7 pouces d'air, tandis qu'une "égale quantité de minium lui en avoit fourni 34. M. "Hales a encore remarqué que le phosphore ou plutôt "le pirophore de Homberg diminuoit le volume de l'air "dans lequel on le brûloit;—que les végetaux en fermentation produisoient d'abord une grande quantité "d'air qu'ils absorboient ensuite. Quant a la com"bustion du volume de l'air qui s'opere pendant la com"bustion de quelques corps, tantôt il l'attribue a la perte "de son elásticité, tantôt il semble croire que cet air est "réellement fixé et absorbé pendant la combustion. M. "Hales termine son sixième chapitre, en concluant que "l'air d'atmosphère, entre dans la composition de la plus "grande partie des corps, qu'il y existe sous forme solide, "depouillé de son elásticité et de la plus grande partie "des proprietés que nous lui connoissons, que cet air est, "en quelque façon le lien universel de la nature, qu'il

moir has neglected the rights of other chemists.

He did not formerly overlook them.

He mentions Hales in his opuscules.

Who ascribing the increase of M. calls it air.

—and considered it in combustion.

—and taught that it may be fixed, &c.

O 2 "est

" est le ciment des corps, que c'est à lui qu'est dû la
" grande dureté de quelques-uns, une grande partie de
" la pessanteur des autres ; que cette substance est com-
" posée des parties si durables, que la violence du feu
" n'est point capable des les altérer, et que même après
" avoir existé pendant des siècles sous forme solide : elle
" peut, redevenir un fluide élastique rare, tout sem-
" blable a celui de notre atmosphere.*

Statement of Priestley's discoveries, by Lavoisier.

" M. Priestley a reculé beaucoup plus loin que M. de Smith, les bornes des nos connoissances ; et on lui est redevable de quelques faits qui semblent decouvrir un nouvel ordre decouvrir de choses†. Il suspendit des morceaux de plomb et d'etain dans des volumes donnés d'air, et fit tomber dessus, le foyer d'un verre ardent. L'air se trouva diminué d'un quart : la portion qui restoit ne fermentoit plus avec l'air nitreux, elle étoit pernicieuse aux animaux, et elle n'étoit plus susceptible de diminuer par un mélange de soufre et de limaille de fer‡. M. Priestley a essayé de calciner les métaux dans l'air inflammable, dans l'air fixe, et dans l'air nitreux, sans pouvoir y parvenir ; mais il a observé qu'ils pouvoient encore se calciner dans un air où le charbon ne brûloit plus. M. Priestley explique tous ces phénomènes par l'émanation du phlogistique ; cette substance qui se dégage du charbon qui brûle et des metaux qui se calcinent, se combine, suivant lui, avec l'air et diminue le volume‖. Ces éxperiences de M. Priestley ont été publiées a la fin de l'année 1772 ; il y avoit déjà du temps que je m'occupois du même objet et j'avois annoncé dans un dèpôt fait a l'àcadémie des sciences le premier Nov. 1772, qu'il se degégeoit une énorme quantité d'air des reductions metalliques.§"

These passages are inserted in Lavoisier's last memoir.

Had these passages found a place in the memoir, and had we also met with a similar history of Priestley's discovery of dephlogisticated air, and his observations on respiration, we might have become the earnest partisans of an injured philosopher, and exclaimed " this theory is " not, as we often hear it called, the theory of the French " Chemists¶—it is the theory of Lavoisier. Others

* Lavoisier Opuscules, 25. † 87. ‡ 142. ‖ 143. § 109.
¶ Nicholsons's Journal xiii. 85.

" have

" have contributed to its perfection, but the present " theory of oxydation, combustion, and acidification is " Lavoisier's."

Priestley did not theorize but adhered to phlogiston.

Priestley made the greatest discoveries, but not having previously studied chemistry, he in this instance surrendered his understanding to the guidance of an established sect, and admitted the phlogiston of the Stahlians as an elementary principle of bodies; and he was more readily led to continue a follower of the prevailing system of chemistry from having detected an error in the absorbent theory of Hales*.

Lavoisier saw the better part of Hales's theory.

Lavoisier adopted the better part of Hales's doctrine and though he did not at first reject the phlogistic system, he early suspected that charcoal performed other offices in the reduction of metallic calces, besides that of restoring phlogiston to the metallic calx. Whether he or M. Bayen ventured first to deny the existence of phlogiston I cannot ascertain, but previous to the 7th Dec. 1773, Lavoisier expressed himself on the subject as follows:

Whether Bayen or Lavoisier first denied phlogiston?

" Cette derniére observation nous conduit a des reflex-" ions sur l'usage du charbon̄ et des matières char-" boneusses en général dans les réductions metalliques. " Servent elles comme pensent les disciples de M. Stahl, " a rendre au metal le phlogistique qu'il a perdu? ou " bien ces matières entrent elles dans la composition même " du fluide élastique? S'il étoit permis de se livrer aux " conjectures, je dirois que quelques expériences, qui ne " sont pas assez complettes pour pouvoir être soumisses " aux yeux du public, me portent a croire que tout " fluide élastique résulte de la combinaison d'un corps " quelconque solide on fluide, avec un principle inflam-" mable, ou peut-être même avec la matière du feu pur, " et que c'est de cette combinaison que depend l'état " d'élasticité: j'ajoutcrois que la substance fixée dans " les chaux metalliques et qui en augmentent le poids, " ne seroit pas, a proprement parler, dans cette hy-" pothèse un fluide élastique; mais la partie fixe d'un " fluide élastique, qui a été depouillée de son principe " inflammable. Le charbon alors, auroit pour objet

* Priestley on Air, i. 132.

" principal

"principal, de rendre au fluide élastique fixé le phlogis-"tique, la matière du feu, et de lui restituer en même "temps l'élasticité qui en depend*." M. Bayen published his Experiments on Mercurial Precipitates, in the Journ. de Phys. for 1774, from which, as far as I can judge from the notes which I formerly made when I perused them, he drew conclusions similar to those of Lavoisier. Dr. Thomson in his Chemistry I. 101, ed. 2nd, says, that it was in consequence of hearing Bayen's paper read that Lavoisier was induced to turn his attention to the subject, which must surely be a mistake, as Lavoisier's experiments† on the same subject, were printed at least previous to the 7th of Dec. 1773. Dr. Thomson on examining the subject will I think find that he has committed another error in the same note. After relating in the text that Lavoisier revivified the calx of mercury by heat alone, he adds in the note that "this experiment was "performed by Mr. Bayen in 1774." I am led to believe from my notes that Dr. T. will find that the account of the experiments was at least published in 1775, as you state in your Journal xiv. 233, note.

It appears that Lavoisier was the first.

Short remark on Rey and Mayow.

As for the theory of Rey, I should be happy to peruse his book as an object of curiosity, but I do not wonder that his opinion should have excited little attention, when he alledges that "the increase of weight arises," from "the air of the vessel condensed, rendered heavy and "adhesive by the violent and long-continued heat of the "furnace‡." For my opinion of Mayow I must take the liberty of requesting your correspondent E. D. to peruse my remarks on his discoveries in the Med. and Phys. Journ. iii. 335 §.

I am,

Sir,

Your very obedient servant,

JONATHAN STOKES.

* Lavois. Opusc. 280. † 247.

‡ Journ. xiii. 82.

§ But qu. as to Hooke's claims in the theory so fully stated in his Micrographia and copied in our quarto Journal?

IV.

IV.

Extracts from a Collection of Papers on the Subject of Athletic Exercises. By Sir John Sinclair, *Bart. M.P.*

[Continued from p. 77.]

THE case of Doctor Taylor of Croydon, narrated by Dr. Cheyne, is an instance of the power of regimen in eradicating one of the most terrible diseases incident to human nature. That gentleman had for many years been afflicted with the epilepsy to such a degree, as frequently to fall from his horse in the course of his business, and remain insensible on the road till picked up by the next passenger. Having observed that the lighter his food the less frequently did his fits recur, he confined himself wholly to bread and milk. This diet occasioning flatulence, he restricted himself to milk alone, of which he took about two quarts per day. Under this regimen he completely recovered his health and strength, so as to be able to play at cricket for many hours together, with hardly any perspiration. During fourteen years he experienced no recurrence of his fits, and at length died of a pleurisy, occasioned by cold caught from sleeping in a damp bed. I had once an opportunity of seeing this regimen adopted in a deplorable case of the same malady. The disease was not indeed cured, though much mitigated, and during the year it was persisted in, the patient considerably recovered his health and strength.

Doctor Taylor of Croydon was cured of epilepsy by milk diet.

When it is considered that persons most conspicuous for elegance of person, as well as for acuteness of intellect, are peculiarly liable to become the victims of the sure though slow-moving dart of phthisis pulmonalis, it becomes a very desirable object to possess some means of opposing the depredations of that scourge of our island. To effect this purpose it is requisite to be able to detect the earliest advances of that insidious disease. To discover any remedy that will remove tubercules, or cure ulceration of the lungs, if actually present, judging from the analogy of other diseases, is hardly to be expected. Without quibbling about the term hereditary disease, no doubt can remain in the mind of any man of observation,

Observations on those subjects who are peculiarly liable to pulmonary consumption.

2 that

that children are not only prone to the diseases of their parents, but are even peculiarly liable to the diseases of that parent to whom they bear the closest personal resemblance. That children particularly resemble their parents in the structure and formation of their nails, has not been so generally remarked; and a child will very generally be found to partake most of the constitution, and consequently to be peculiarly liable to the diseases of the progenitor to whom it has the greatest similarity in this particular part of the body. A certain conformation of the nails affords also a strong indication of the disposition to phthisis. In persons of the consumptive habit, the nails are in general large, long, of a fine texture, and curved over the ends of the fingers, the last joint of which appears as if enlarged or thickened. When this peculiar structure of the termination of the superior extremities is found combined with fine sound teeth, a flaccid skin and high shoulders, little doubt can remain of the existence of disposition to phthisis, whether the individual be of a fair or of a dark complexion; and if we find that any hereditary taint is present in either of the parents, it is almost certain that their offspring will ultimately become the victims of this disease.

Remarkable similarity of children to their parents in the form of their nails.

Description of that structure which indicates a disposition to consumption.

It may be subdued by regimen.

Of the effects of a regimen of the farinacea, combined with milk and fruits, in subduing the early attacks of phthisis many examples are recorded; and there would probably be many more, were an appropriate regimen adopted rather with a view to prevent than to cure this disease. Hence the utility of noting every mark that can lead to the detection of a tendency to this disease, and the consequent adoption of a plan calculated to prevent its earliest attacks.

More may be done in this respect than has hitherto been supposed.

Some experience has induced me to be of opinion that more may be done to counteract the predisposition to this disease than has hitherto been effected. The surface of the lungs and that of the skin are both secreting organs, the functions of which mutually compensate each other; a languid and inert condition of the skin is necessarily attended with a diminution of cutaneous perspiration, to make up for which a larger share endeavours to escape by the lungs, and this increased effort may well be supposed to lay the foundation for disease. The hypothesis

thesis is supported by the well-known facts that sailors, ploughmen, butchers, and all persons whose occupations are carried on in the open air, and whose perspiration is therefore free and copious, enjoy a remarkable exemption from pulmonary complaints; on the contrary, two thirds of the working tailors of London, taking them as an example of the sedentary class of artificers, are believed to die of pulmonary consumption. Let us then endeavour to remove this inert condition of the skin, not by internal sudorific medicines, which would only relax it more, nor by keeping the body constantly bathed in an atmosphere of its own perspiration by casing it in flannel. Rather by daily exposure to the air bath, during which the surface of the body should be rubbed with a hard flesh brush, either by the hands of the patient, or by those of an assistant till the whole skin glows. From a sedulous attention to this practice, which when regularly persisted in becomes very grateful; combined with a light dry diet, and unremitted exercise in the open air, I have seen such an alteration produced in the constitution, as leads me to hope that much may be effected in repelling the attacks of this disease, if the proper means be sufficiently early employed. — principally by an attention to the state of the skin.

Should this sketch of the mode of training the antient *Athletæ*, which suggested these few hints concerning the influence of diet, air, and exercise, in counteracting certain diseased states of the constitution, coincide with your plan of diffusing a more general knowledge of the means of preserving health, and preventing disease, I trust you will accept of them as a mark of my respect for that wish to ameliorate the condition of mankind, which appears on this occasion to have directed your efforts. Conclusion.

I am, Sir,
Your most obedient servant,
A. P. BUCHAN.

Percy Street, London,
20th March, 1806.

P.S. The preceding observations being intended to indicate the physical changes possible to be effected in the human constitution, by a peculiar course of diet and exer- The moral effects of boxing, &c.

 cise,

cise, combined with exposure to a pure air, persisted in, during a time given, all remarks on the moral effects of pugilistic exhibitions, to which such a course of training, forms a necessary prelude, have been intentionally avoided. This subject has lately been discussed with equal acuteness and propriety by Dr. BARDSLEY, in "A Dissertation on the Use and Abuse of popular Sports and Exercises," published in the last volume of the Memoirs of a Literary and Philosophical Society at Manchester, which, had it fallen into my hands before this account of the mode of training the antient *Athletæ* was transmitted to you, would have saved me considerable trouble. The reader will there find the different effects produced on the public mind by the exhibitions of human prowess, displayed in the practice of boxing, well discriminated from the consequences of committing acts of cruelty on the inferior animals, such as bull-baiting, throwing at cocks, and other execrable practices of a similar kind, which have been most improperly denominated sports. Ferocity of manners, and brutality of conduct, are the invariable consequences of indulging a propensity to witness such exhibitions. In England, where the art of boxing is particularly exercised, the number of persons who fall sacrifices to personal quarrels, or become the victims of resentment, are few indeed; whereas, it has been calculated, that at Rome a thousand persons are annually murdered by the stiletto of the assassin, and the proportion is probably not less in Spain and Portugal. In the southern counties of England, where the mode of deciding private quarrels among the common people, by an appeal to manual combat, is peculiarly prevalent, instances of their terminating in death are very rare. In the northern counties, on the contrary, where, when men fight, they take every unfair advantage, the loss of life is by no means uncommon, and the verdicts of man-slaughter occur so frequently as to have repeatedly excited the indignation of the judges. It is even stated, that since the practice of fair boxing has been in some measure introduced into the northern parts of this country, by the example of the itinerant teachers of the pugilistic art, instances of murder have become less frequent.

—have been well discussed by Dr. Bardsley.

Fair combat produces generosity and less mischief

—than vindictive struggles.

The pain inflicted and suffered by the persons engaged in these contests have caused them to be stigmatised by many humane persons, as cruel. But to judge of the feelings of the combatants by those of the spectators, is a very inadequate criterion. It is an acknowledged physiological truth, that the simultaneous actions of voluntary exertion, and of sensation, are in great measure incompatible with each other. Hence the utility of the bullet in the soldier's mouth, who is suffering punishment: by strongly exerting the muscles of mastication on this unyielding substance he diminishes the sensation of pain. Even our immortal bard appears to have been acquainted with this fact, when he makes Henry the Fifth desire his soldiers, previous to mounting the deadly breach, to

The efforts of combat diminish the sense of pain.

> " Stiffen the sinews, summon up the blood;
> " To set the teeth, and stretch the nostril wide,
> " Hold hard the breath, and bend up every spirit,
> " To his full height."

Such is the condition of persons engaged in a boxing match. And many who can contemplate the gallantry and spirit displayed in a contest of this kind, with considerable satisfaction, would shrink with abhorrence from the spectacle of a man beaten in like manner, were he at the same time deprived of the power of resistance.

It is also matter of common observation, that a man, in possession of a robust and vigorous constitution, suffers much less pain from a certain degree of injury than a person in a more feeble state of health. Carry this a little farther, and we find a delicate lady, whose flaccid muscles hardly suffice to support a debilitated frame from one chamber to another, yet highly susceptible of pain from the slightest external injury, and suffering almost annihilation at the sudden clapping of a door. It would seem, therefore, as if the force and irritability of the muscles, and the susceptibility of the nerves were in the inverse ratio of each other. The effect of a course of training appears to be to augment the quantity, and irritability of the muscular fibre, and at the same time, to diminish the morbid sensibility of the nervous system. And I think it is advancing a step in physiological know-

Robust constitutions suffer less from violence than the weak and irritable would do.

Training gives this vigour.

 ledge,

ledge, to have ascertained the means of augmenting or diminishing these opposite states of the living body.

Argument respecting pecuniary motives.

It might indeed, be desirable, that the persons who engage in these public contests were less influenced by pecuniary motives, and that there was less of the spirit of gambling connected with them. A rigid adherence to the rules of the combat, which chiefly consists in abstaining from taking any unfair advantage of an antagonist, is particularly attended to in a regular boxing match, and by such examples this generous feeling is supported and diffused among the mass of the people. Were the victor not rewarded by some prize, or testimony, of his superiority, the practice would probably soon fall into disuse.

If boxing were attended with neither pain nor danger, the discipline would not produce that steady courage which our countrymen have so often displayed.

Dr. Bardsley has proposed, in order to prevent injuries, that these combats should be carried on in mufflers, or stuffed gloves. The preparatory exercises, it is well known, are always managed in this manner. But an important part of the practice of boxing consists in that cool and steady courage which enables a man to endure a certain degree of corporeal suffering with patience; and in a command of temper, which preserves the presence of mind, undisturbed, amid pain and danger. This cool intrepidity constitutes the characteristic feature of British bravery; and whatever may be the evils attendant on prize-fighting, they are more than compensated, if it be allowed that such exhibitions tend to diffuse and support a spirit of this kind, among the inhabitants of the British Isles.

V.

Reverie; considered as connected with Literature. By the Rev. Johnson Grant, A.B. *of St. John's College, Oxon. From the Manchester Memoirs, Vol. I. of the Second Series*.*

Explanation of reverie, or the act of the mind in which surrounding objects and immediate pur-

IT is a frequent process, and often one of the highest pleasures of the mind, to become insensible to the pursuits in

* Though the present memoir cannot with the most decided propriety be considered as belonging to Natural Philosophy, Chemistry,

in which it is more immediately engaged, and yielding to impressions which lead to more interesting trains of ideas, to suffer itself to be carried by them to an imaginary contemplation of distant scenes, or speaking over of former conversations;—to a recollection of past transactions or anticipation of future enjoyments. This mental observation is known by the name of Reverie: and is also expressed in common conversation by the emphatical metaphor—absence of mind.

suits are disregarded for more interesting musing.

Without entering into the question how far volition is concerned; whether the mind is active or passive in a state of reverie, it will not be improper, for the sake of imparting clearer ideas on the subject, to draw a parallel betwixt reverie and abstraction, according to the common acceptation of the terms. Abstraction is the act of attending closely to the object of study, which is present to us:—Reverie is the state of being drawn away from an observance of that object, by other reflections. The one indicates strength; the other a degree of weakness of mind:—abstraction is an effort to collect our thoughts: reverie consists in their being let loose, to wander whithersoever they will. Abstraction is a steady and continued act of pondering on the object before us. Reverie, as it is to be considered in this paper, consists in a want of the power of abstraction.

Reverie differs from abstraction.

In abstraction the mind is strongly fixed by choice on one present subject: in reverie it is weakly surrendered to the wanderings of fancy.

It is a want of abstraction.

Abstraction resists the impulses of external objects, which have a tendency to disturb the train of ideas in study. Reverie surrenders the mind to these impulses, and to the new train of ideas, (foreign to the immediate subject of contemplation) which they introduce. Abstraction is peculiar to the philosopher:—reverie to persons of sensibility and genius, uncorrected by strength of mind. Abstraction is the habit of the diligent. Reverie, the trifling of the idle*.

Powerful minds dwell by abstraction on their studies.

Light minds are diverted by reverie.

Every

mistry, or the Arts, yet its importance to all scientific and studious men leaves me no doubt that it will be acceptable to the readers of this Journal, whose progress in knowledge, and consequently the advancement of the sciences, must be promoted in proportion as their researches are guided by sound reason and an attention to the processes of the mind.

* It may, in some cases, at first seem doubtful, whether to refer certain

Manner in which the mind becomes absent, &c.

Every man is conscious that his mind is often imperceptibly conveyed away from the objects that are presented to his senses, and led to other catenations of ideas. Among these it ranges for some time, till at length, in a manner apparently inexplicable, it perceives itself brought back to its immediate employment: but is equally at a loss to explain how it broke loose, how long it has been absent, or what has occasioned its return.

Medical views.

Physicians, who have treated this mental infirmity as a disease, have confined themselves to a description of the constitutional frame, which renders us liable to it *. Having

Reverie of the Poet.

certain operations of the mind to the former or the latter of these terms. Poetry is one example—But a little reflection will solve the difficulty. Some of the poets' finest ideas may be derived from reverie:—but to embody them in words, to give them a local habitation and a name, close abstraction is certainly required.

Two kinds of reverie.

It may, also, be proper to observe, that there are two distinct species of reverie; each of which interrupts study. The one is unconnected with the object of our study, and is occasioned by a strong impression on our mind, which disturbs the power of attending to another subject: as if after witnessing an execution, I should attempt to read a book of philosophy, the horrid spectacle would in this case intrude upon my thoughts, and render attention impossible. The other species arises from the subject, and is frequently produced when the mind is at ease. Cato's Soliloquy on reading Plato's Treatise on the Soul's Immortality, may be supposed to be an example of it:—and this species of reverie may easily be confounded with abstraction.

The sensorial power employed on reverie diminishes irritability,

* Dr. Darwin, vol. I. p. 361, says that " people with increased sensibility, who may be known by high coloured lips, dark hair, and large eyes, are most liable to enthusiasm, delirium and reverie. In this last affection, they are seen to start at the clapping of a door, because the more intent any one is on the passing current of his ideas, the more is he surprised at their being dissevered by external violence. But owing to the great expenditure of sensorial power on these sensitive motions, it follows, that there will be a deficiency of it in the irritative, which will be performed with less energy.

— and renders us less sensible to surrounding objects, &c.

" Hence these persons do not attend to slight stimulus; but when a stimulus is great enough to excite sensation, it excites greater sensitive actions than in other constitutions. This is the case in delirium or inflammation.—Thus persons addicted to reverie are absent in company;—sit or lie long in one posture, and in winter have the skin of their legs burnt in various colours by the fire. They

ing omitted to analize the method, by which mind and body act and re-act on each other, they have failed to trace the disease to its source; and in point of remedy, have left it where they found it.

When the attention is engaged by abstraction, the organs of sense become inactive;

In the course of reading or reflection, the subject which engages us may be a task, or a pleasure; it may either be indifferent to us, or deeply interesting. If it be of the latter description, (or even in the case of the former, if we happen to possess a strength of mind) attention will be collected from every quarter, where it may usually be dissipated, and gathered to this single focus. It seems to leave the organs of sense;—which, hence, become callous to impressions, at other times forcibly perceptible. A bell may toll, and the hail may rattle on their windows; but both may be alike unheard. To this state of the mind, philosophy gives the name of abstraction.—If, on the contrary, I have a more favourite study than that in which I am engaged; or if, when I am engaged in study, there be some pleasure which I expect or have lately enjoyed; or some misfortnne which I apprehend, or have lately sustained, dwelling upon my mind: I shall find it difficult to fix my attention—my thoughts will be perpetually recurring to this more interesting subject; my inclination to wander, and my desire to improve will carry on an equal contest; and I shall discover, on laying aside my book, that I have been reading one thing, and pondering on another. This double operation of the mind, constitutes that species of reverie which is peculiar to literary persons.

—but some prevailing object may draw the mind into reverie.

External impressions may draw the mind from its subject.

Our train of thought is disturbed, when any of our senses is acted upon by some quality in an external object, which tends to introduce a new series of reflections.

They are fearful of pain; covet music and sleep;—and delight in poetry and romance." As the motions excited in consequence of increased sensation, are more than natural, and thus expend a greater portion of sensorial power, the voluntary motions, like the irritative, are less easily exerted—Hence the persons we have been describing are indolent with regard to all voluntary exertions, whether of mind or body. They are also known by interrupting others in discourse with irrelevant observations. Deaf people adhere longer than others to one subject, as their train of ideas are in no danger from one inlet of disturbance.

Thus,

Thus, distant music may draw away attention from the book we peruse, to a scene, where the same sounds were formerly heard by us. Or, in the course of reading, we may meet with a passage, which suggests reflections irrelevant to the main subject. From these, when the mind is conveyed to them, the transition is easy to others, with which they are connected; and in this manner fancy may rove, for an unlimited time, through an unlimited range of ideas. The ocean, for example, may be introduced as a simile, illustrative of a metaphysical argument. Fancy will be drawn for a moment to the ocean, and if we have ever beheld it, or crossed it, the incident will present itself.—We then insensibly relinquish our employment to think on the storm which endangered our life; or on the country and friends, from whence the vessel conveyed us. May not a similar process rouse us from this trance, and recal us to the occupation we had left?—May not a new and unusual impulse upon any organ of sense, startle and remind us, that we are trifling with time?—May not the train of ideas, furnished by the reverie itself, lead us back to the very subject which engaged us, prior to its commencement?—In either way the reverie will be terminated. The firing of cannon may break in upon my fit of absence. When once awakened, but not till then I become conscious that I have been guilty of relaxation from the vigilance of attention, and return to my study, pleased, perhaps, with the excursion, but not without dissatisfaction on account of my loss of time. The same effect may be produced in the instance of the simile already mentioned, if the associated ideas to which the simile of the ocean had led me, taking a retrograde direction, conduct me back to the primary subject of comparison.

Instance of mental wandering.

Reverie may terminate itself, or be broken by external events.

The look and action of an orator fix the attention and prevent reverie.

When listening to the discourse of an orator, or lecture of a teacher, we digress in a similar manner, and are recalled by a look from the speaker;—by a pause;—by a sudden transition; a new figure; or a felicity of diction or of thought. This reflection may serve to analize the art of keeping attention awake in others.—It may recommend the impressions we have enumerated, as useful expedients in oratory; and explain the principle, which makes

makes us wish to have a public speaker in our view, while we are listening to him.

When the habit of mental absence is sufficiently confirmed to constitute a disease, the appulses of external objects, which would interrupt reverie in stronger minds, are found to strike upon the senses in vain. A man is mentioned in Zoonomia, who, during the paroxysm of reverie, was reciting some lines from Pope, one of which he had forgotten: it was several times ineffectually shouted in his ears; till at length, after much labour, he recollected it by his own efforts. Yet though such appulses do not destroy, they sometimes harmonize with the waking dream. In this case they excite attention; and the reverie, without being broken, insensibly glides into subjects connected with these appulses. In the work we have just now quoted, is an interesting account of a young person, who, while lost in reverie, heard a passing-bell; and without being recalled to a consciousness of wandering thought, was soon after heard to say, "I wish I were in my grave;"—and pulling off her shoe—"A little longer and a little wider; and even this would make a coffin."

Mental absence in the extreme is a disease

—which is not removed by external impressions;

—though they may be perceived.

Such are the various kinds and degrees of reverie. The enumeration of them was necessary to the discovery of those means by which this mental affection may be regulated or remedied. The subject is of the highest importance to those who are entering upon their studies; since, as it is an argument against wasting much of our time in sleep, that we may be said only to live while we are awake;—so, with regard to letters or business, it may be asserted, that we do not study all the hours we number at our desk, but those only, during which the vigour of our minds has been exerted in our proper employment.

It is of great importance to studious men that they should not indulge reverie.

There are several methods by which reverie may be regulated and modified.

1st. The abstraction of excitement produced by external stimuli, will, in most cases, give a preponderance on the side of study, and thus be inimical to reverie. A walk along the shore is more favourable to abstraction, than in a garden or terrace, where the frequent turnings interrupt reflection. Philosophers in general have

Reflections on the causes which prevent or which favor reverie.

 shunned

Solitude. shunned the town, that its noise and bustle might not disturb their meditations. Nevertheless, we have heard of some, whose minds were more active amidst the uniform, mingled hum of the throng, or the noise of a carriage, than in more tranquil scenes. This may be Town life. accounted for by asserting, 1st. That such uniform sounds may be from habit, associated with abstraction, as opposed to reverie; and that it is only by sharp, sudden impulses, and not by uniform and accustomed sounds, that abstraction is discomposed; and 2dly, That, when this is the case, the sounds in question will drown all others, and thus weaken the influence of their excitement in disturbing abstraction, and producing reverie. Here, Whether retreat be most favourable to reverie or to abstraction. however, a difficulty arises. If I remove myself to silence and solitude for the purpose of philosophical abstraction, should reverie by any means find its way to my mind, and experience proves that no silence and no solitude can exclude it,—will not the absence of excitement from external objects be favourable to the continuance of those idle musings, which I have taken pains to avoid? The first object of a student is, to preclude the advances of reverie; but when its spell has stolen upon him, external stimuli become desirable in order to dissolve it. Hence a retreat into the shade will only facilitate reverie; unless we carry along with us a fund of information, on which we may ruminate: an object of science to occupy and interest us; and an inherent vigour of mind, which shall enable us to resist the slighter impressions on our senses, from which Second sight of the Highlands of Scotland. the deepest retreat is not exempt. The superstitious dreams which are known by the name of second sight, are found amongst the most uninformed of mankind, in a country where the absence of disturbance might favor the highest speculations in science. The beach of the sea, which Plato chose as the fittest place for philosophical instruction, has in our own country become the favourite haunt of the indolent and the unthinking.

Reverie is lively or serious according to its cause. Objects and circumstances may be so disposed as to give to reverie a pleasing or pensive, and as we shall presently see, a refined or inelegant direction. I believe it is unnecessary to ask, whether the mind will not be more apt to depart from serious meditation in a gaudy chapel,

 than

than in the solemn gloom of a cathedral. It is remarked by an eminent medical writer, that light, introduced by opening the window-shutters, gave a gayer cast to the ideas of a patient who laboured under reverie. The study of Tasso was a Gothic apartment; and he fancied his familiar spirit to converse with him through a window of stained glass.

If we can contrive to effect, during the reverie, a frequent re-action of any circumstance connected with our original employment, we shall, by this means, frequently bring back the mind from its excursion. It has been asserted, in favour of the liturgy of the Church of England, that, by being broken into short prayers, and interspersed with frequent responses to be spoken by the people, it is accommodated to the frailty of human nature, and has proved an excellent method of recalling the mind, too apt to wander, even from its most important occupations, and its most sacred duties.

The English Liturgy is calculated to fix the mind, and prevent reverie.

A house of worship is certainly the most suitable place for acts of devotion. The mind is no sooner inattentive, than it sees around it objects connected with religion, which upbraid its weakness and check its aberration.

I now come in the last place to enumerate the remedies I would propose for the diseased state of the mind, which has been the subject of the present dissertation—and these all rest upon a single principle. The "vis insita" of the mind, inclining, by a voluntary exertion to the side of study, constitutes the power of resisting the seducements of external stimuli, and of bidding defiance to reverie:—and as reverie has been shewn to proceed from mental relaxation and debility, so, whatever produces mental vigour may be pronounced an antidote to it.

Study is a remedy for this mental defect,

Now mental vigour is, in great measure, regulated by the strength of the body; so that literary persons, who are desirous to preserve their minds in a proper disposition for studying with the greatest benefit, should remember, that with respect to exemption from reverie, it is only "in corpore sano" that the "mens sana" is to be found.

—and so is bodily vigour.

The first remedy accordingly which I shall mention is, frequent and habitual exposure to a pure and bracing atmosphere,

This is promoted by exposure to the open air,

to read in his tent, at midnight when his frame was debilitated, and his spirits were exhausted by a long march, and by the heat of the morning;—when his mind was unstrung, and prevented by weariness from exerting its powers with one fixed direction. May not the spectre have been a creature of his imagination when thus predisposed for reverie? when his ideas consisted of confused conceptions, furnished partly by his book and partly by his fancy. And will it be deemed extravagant to conjecture, that the passage he was reading may have been the story of the dying Bramin, who prophetically warned Alexander that they should meet at Babylon?

The spectre which appeared to Brutus.

Fact supposed contrary to this.

I am aware, that the mind, when deeply engaged in study, sometimes overcomes sleep, and assumes a new vigour at a late hour of the night. In this case, certain degree of fever, in other words, of increased action, has taken place; which will be followed, and proved to have existed, by commensurate mental debility and nervousness.

"Some," says a modern author, "look over what they want to remember, immediately before going to sleep at night, because then the mind is not afterwards busied about any ideas that might drive it away: or in the morning on first getting up, because the mind is not then pre-occupied with any ideas which may hinder the subject's getting fast hold of it."——*Gerard's Pastoral Care.*

*On the whole, whatever destroys the balance between body

Diseased sensibility and impaired muscular vigour are concomitant.

* It is a law in the animal œconomy, that sensibility accumulates as irritability is exhausted: in other words, that the nervous fibre becomes more sensible to impressions, as the muscular fibre becomes less so, and vice versa.—Preternatural or diseased sensibility is not found in the strong labourer so much as in the hysterical and debilitated female. The author of this essay, who can encounter without mental pain, any scenes of distress which he may witness in his professional character, in the morning, when the frame is in tone, has observed in himself a propensity to be much affected by them, when presented to him after fatigue and long fasting. Whatever accumulates sensibility, increases the mind's liability to be acted upon by external stimuli, and carried away by them from its steady observance of the object of its study. And since the exhaustion of irritability produces this effect, the propriety of the foregoing injunctions is evident.

body and mind, whatever impairs the firm tone of the animal fibre, ought to be studiously avoided by those whose habits are literary. The debility subsequent to a debauch, a warm climate, fatigue, corpulency, are all favourable to reverie. And every thing that braces the fibre, and gives the system (not a sudden and artificial increase of action,) but permanent strength and exhilaration should, with equal care be resorted to.

Literary men ought to avoid all causes of debility, for these favour reverie.

But for the mental disorder, which has been the subject of our discussion, we must look, in the second place, for other remedies in the mind itself, when considered abstractedly from the body.

Mental remedie against revie.

Much benefit will be derived from conquering a sickly taste for light and desultory reading, and abstaining from an immediate application to the fine arts. When they, who have indulged in such pursuits, engage in studies of more solid utility, they find the perusal of historic facts, or the prosecution of philosophical arguments, perpetually interrupted by the involuntary remembrance of their favourite and less severe employments. Mathematics is a science worthy of being recommended to youth, and, indeed, demanding the attention of all whose habits are literary; not so much for its own sake, or for that of the other sciences which cannot be understood without a knowledge of it, as on account of its implanting habits of abstraction and of bestowing the ability to fasten the powers of the mind upon any subject, and to pursue it till it is thoroughly investigated*.

To avoid light reading.

Mathematics recommended.

Here, however, a caution is necessary. Elegant literature and the fine arts, although thus paralysing to the mind when they are made the main object of pursuit, may in certain cases be called in with advantage, as remedies for reverie. When the mind is under the influence

But elegant literature may be considered as an exhilarating refreshment.

* In comparing the effects of the different leading branches of education at our two universities, it has been remarked, that persons who have studied at Cambridge, adhere long and steadily to an argument, in conversation; while Oxonians, whose pursuits are more elegant than philosophical, are content with a more superficial examination of many subjects; but afford greater pleasures to their companions, by the desultory variety of the ideas which they communicate.

Oxford and Cambridge students compared.

of

of any passion, joy, surprise, grief, indignation, which deprives it of the ease and exemption from solicitude requisite to its applying with effect to abstruse researches or what is called serious reading,—it will then be its philosophy to lure attention into the paths of literature, with the elegant classic, or interesting narrative;—with the works of poets or dramatic authors; and with composition on its favourite theme:—stimulants powerful in calming the soul, and charming sorrow into tranquillity, when rarely and prudently applied; but which would lose their effect, if they were daily administered.

Objects which seduce attention are to be avoided.

Another expedient, which it will be prudent to adopt, is the removal of our place of study, beyond the reach (if possible) of every object and circumstance which being presented to any of our senses, is apt to seduce attention. The fragrance of flowers, the voice of music, the portrait of a friend, the hum of men, has each its train of associated ideas, to pursue which, the mind of the student may insensibly be drawn off from the object of his study. And if the student wishes to obtain a depth of thought, a closeness of reasoning, dispatch, or perfection in study, he will reserve these luxuries for the hour of relaxation. It was one of the maxims of Lycurgus, that ornaments should not be placed in the council halls, as they tended to alienate the attention of the judges, when listening to the pleaders.

Art of memory.

The art of memory has been said to be the art of attention;—the art of preventing the operations of the mind from being broken by short reveries, to which weak minds are decoyed by every sound or sight that passes. It is possible for a Newton to be so deeply absorbed in thought, and to have practised abstraction so thoroughly, that the firing of a cannon will not break the train of his ideas.—

Remarks on men supposed capable of abstraction in all situations. Newton, Colbert.

But common minds, conscious of their inferior strength, and of their greater aptitude to be interrupted, should cultivate letters in places where the fewest and the weakest stimuli are applied;—in the shade, remote from noise, and not exposed to passing objects. Colbert's having said that his mind was always most active in the midst of Paris, if not fully solved in the former part of this essay, may be considered as a proof, that that minister possessed a warm imagination,

imagination, guarded by a vigorous intellect;—that he was willing to give loose to the wanderings of fancy, in the midst of rural leisure: but ever associated the recollection of want of time and fulness of occupation in the metropolis, with the first aberration of thought from the subject he had before him.—Besides, it is reasonable to suppose, that the studies of Colbert, when in Paris, were confined to the politics of the day; a subject which, by engaging every passion, must have entirely engrossed attention, and deadened the force of external stimuli: whereas his rural lucubrations had, probably, for their subject, topics of speculative philosophy, less interesting, less relating to self and immediate concern; and therefore less endowed with the power of detaining the mind, prone to her favourite sallies of digression from her main employment. Nothing can be more absurd than an attempt to unite a life of literature and of gaiety.—The remembrance of glaring objects and tumultuous pleasures, perpetually obtruding itself on the mind, will soon convince the scholar, that his efforts to make thought and dissipation of thought meet in the same mind are vain.—The recollection of past, or anticipation of approaching frivolities, makes abstraction a painful and violent, I may safely affirm, an impossible exertion. The conceptions of an effeminate imagination unsettle the mind;—they float upon and confuse the ideas supplied by study.

Literature and diversions cannot be combined.

Indeed a habit of study and abstraction is the most powerful precaution that can be adopted against the intrusions of reverie.—Reverie resembles the enemy of mankind. Resist it, and it will flee from you. The oftener and the more vigorously you oppose it, the less frequently will it recur, and the weaker will be its attacks. While the idler and the man of pleasure cannot peruse even a few pages of a novel without mental weariness and wandering;—the student will in time bring his mind to the ability of prosecuting for many hours, the deepest reasoning, seldom interrupted by reverie, and never overcome.

Habits of abstraction gradually destroy the habit of reverie.

When speaking of the force of habit, we cannot fail to recommend the habit of extemporaneous speaking. When a man finds that his words must flow in an uninterrupted

Extemporaneous speaking.

succession, and that his ideas must keep pace with them, he will have no leisure for idle musing.

The various circumstances by which attention may be fixed to a subject.

Let us suppose, a contention held between the employment which engages us on the one hand, and the stimuli that act upon onr senses on the other. Each strives to draw the attention of the mind towards itself. If the employment be pleasing, or if several of the senses, instead of one, beengaged in it, we may consider it as the stronger party, as having the greatest force on its side. Attention would be less apt to waver if we were to transcribe, than if we read a passage in any author; if we saw a drama performed on the stage, than if we perused it in the closet; or if we were present at a parliamentary debate, than if it only reached us through the cold medium of a newspaper. When the mind therefore is agitated, and incapable of intense application, it will be well to betake ourselves to any occupation of which we are enthusiastically fond. Whence arises the fluency of the unlearned itinerant preacher. It is to be ascribed to the two last principles on which we have expatiated, habit and enthusiasm.

An expedient to prevent wandering of mind.

It often happens to those who devote much time to reading or composition, that as soon as their reverie commences, they unconsciously remove their eyes from their book or writing desk to some particular spot in the apartment which may be favourable to mental wandering, or associated with it by habit. Now, if they would previously affix to the idea of this spot, the idea of consciousness that they have departed from their proper occupation, they would probably be enabled in this manner to check the fit of musing at its commencement, and to save the time which would otherwise have been squandered. No one is unacquainted with the story of the orator, who could not plead without holding a string in his hand, for the purpose of recalling his wavering thoughts. The biting

Biting of the nails.

of our nails, during composition, may be referred to the same cause. We associate the idea of this practice with that of our first and main employment, so that the former is never present without the latter;—and any new train of ideas obtruding themselves on our study are kept at a distance by the recurrence of the practice alluded to; which

which we have previously identified with the recollection of our original object of contemplation. As nail-biting is intended to fix abstraction, drumming with our fingers is a practice, by which we promote reverie. This it does partly from habit and partly upon a principle already mentioned; namely, that a gentle uniform stimulus draws attention from all others, except such as are sudden and violent; which will dissolve any reverie, however interesting, and however artfully promoted, unless in a diseased state of the mind.

Drumming with the fingers.

If, therefore, we find that this last mentioned practice is favourable to the continuance of our minds in the regions of imagination, we must frequently, when we have greater command over our thoughts, study to connect and blend the practice with internal disapprobation of our indolence.

If, however the habit of reverie have been too deeply fixed in our minds to be entirely eradicated;—or if (as is the case with many) we be unwilling to part with this pleasing weakness, and consider the moments spent in such desultory musings, as the most delightful of our lives, we ought still to be anxious to regulate them in such a manner as to prevent them from being either unprofitablo or criminal.

If reverie be not dismissed it ought to be regulated;

We may hinder them from becoming unprofitable, by cultivating a taste for intellectual pleasures; by habitual application to a variety of branches of study;—and by frequenting the society of the learned or the refined. The reverie, which we cannot conquer, will thus be converted into a rational employment;—for taste and memory will direct it to subjects of science and utility.

or rendered profitable.

The best rules for preventing fits of absence from becoming criminal, will be found in that book, which is the highest authority on this part of the subject. Keep the heart with diligence, for out of it proceed evil thoughts;— the springs of conduct; the issues of life. Be strenuous in "casting down imaginations" that are contrary to virtue; and "bringing every thought into the captivity of principle." The authors of the book from whence these maxims are extracted, were aware, that it was impossible to put an entire en to the influence of matter

The reveries of a virtuous and regulated mind will be without reproach.

over the mind, and to abolish reverie. They knew that as long as the human frame continued in its present condition;—"the corruptible body would press down the incorruptible soul."—They therefore enjoined the purification of the thoughts; in order that whenever matter should exert its influence upon mind, and force it into unconscious deviation from its employment, mind might be invariably led by inclination into the paths of innocent or pious musing.—Quinctilian relates of his son, that in consequence of his strong attachment to letters, no word escaped him in the delirium of a fever, that had not a reference to his favourite occupation. Thus when the scientific mind recovers from a paroxysm of reverie, it has the satisfaction of reflecting that its time has been well employed;—that if it has not been meditating some new effort of its powers, it has, perhaps, been dwelling on some elegant thought, or glowing description treasured up in study, or heard in conversation. And, in like manner, when the reverie of the virtuous man is at an end, he finds, that, while it lasted, he has either been forming a good purpose, or acting over in fancy, a benevolent deed.

if directed to letters or to science they will be contemplated with satisfaction.

Far different trains of thought pass through the imaginations of the ignorant, the vicious, the sensual. If their minds are not mechanically driven to recollections that are full of remorse and bitterness, the highest pleasures of their reveries are the remembrance of some frivolous enjoyment, or anticipation of the pampering of some base appetite. An Apicius will feast again in fancy on the banquet of yesterday. An Alexander's mind will leave the scene which surrounds it; "thrice to vanquish all his foes, and thrice to slay the slain." How far in frivolous minds a human passion will get the better even of devotion, may be seen by referring to our great dramatic bard.—

The vicious and sensual have criminal reveries.

"When I would think and pray, I think and pray,
To several subjects; heaven hath my empty words,
Whilst my invention, hearing not my tongue,
Anchors on Isabel."

Idle and unprofitable reveries may be also broken, by having our study hung round with portraits of heroes and worthies; of ancient and modern authors; of any who have

The ornaments of a chamber may be adapted to remedy this weakness.

have attained eminence or power, by mental activity and perseverance, and are calculated to rouse the slumbering mind to emulation and energy. And in like manner may we dissolve the spell of reveries, into which evil thoughts are apt to enter, by the pictures of a Saviour, or of a departed or sainted friend. Who would not return, with a blush, from whatever criminal conceptions he had hung upon, when he encountered the eye, and fancied that he beheld the frown of personages so sacred?

Reveric may perhaps be allowable as a relaxation.

To propose a total preventive or cure for the disease I have been considering, has neither been my aim nor my wish. The aim would be ineffectual, as long as mind and body depend and reciprocally act on each other, as they do in the present existence.—The wish would be the dictate of that cold philosophy, which seeks to shut up one inlet of those few, harmless delights, that heaven has apportioned to us, and that nature has commanded us to husband. Yet this riot of fancy should be seldom and carefully indulged. If it be sometimes allowable to slacken the reins, with which the mind is held attentive, never let us throw them entirely away;—for though it would be pedantry to suggest, that since moments thus passed, are inconsistent with our active duties, they ought without reservation, to be condemned;—we ought, nevertheless, to beware of every relaxation, which pre-disposes the mind to habitual inactivity.

Few minds can resist stimuli by abstraction; none can do it perfectly.

Stimuli may be increased to so intense a degree, that attention will be compelled to leave the fondest object on which it broods, and to obey their impulse. For although we have read, that Archimedes was solving a problem during the sack of Syracuse, that Newton was often insensible to his meals having been brought before him and removed; that Cicero calmly pursued his studies while his mind was dejected by domestic grief and harassed by public vexation;—yet it is certain, that pain or hunger, fear or sorrow, or joy, or any violent passion, will, in most minds, overcôme the deepest and most philosophical abstraction.

The mind cannot overpower the senses;

Little credit is due to the story of an Italian philosopher's being so wholly absorbed in contemplation, as to be unconscious that he was upon the rack.—Let us call

mind an elegant sentiment of our master of nature, whose works every philosopher who reads them will often have occasion to quote.

> "Oh! who can hold a fire in his hand,
> By thinking on the frosty Caucasus, &c."

Nor is it fit it should.

Philosophers, nevertheless, there are who assert that man may in time become so perfect that his mind shall be unaffected by variations in the state of his body. But even were this improbability to be desired, it surely cannot be expected; for their mutual reliance is at present so great, that it justifies the conclusion, that mind will never become omnipotent over matter, until it shall be altogether independent of it.

VI.

Account of a Crane, with the Description of a Method of working the Common Chain in Machinery, so as to exceed Ropes in flexibility and strength. By Mr. GILBERT GILPIN, *of Old-Park Iron-Works, near Shifnal*.*

The common chain will answer every purpose of a rope.

FROM its simplicity of form, and facility of manufacture, the common chain, formed of oval links, has been in use from the earliest ages; and that it did not answer every purpose of a hempen rope in working over pullies, was not owing to its peculiar form, but from an error in the application.

Reasons why it has hitherto failed. It has a twist,

Every chain of this nature has a twist in itself, arising from a depression given by the hammer to each link in the welding†; and this circumstance, so trifling in appearance, is not so in its effects, and it has in consequence a perpetual tendency (even when reefed perfectly straight in pullies, and on the barrels of cranes) to assume a spiral form, which a plain cylindrical barrel, and the

* Communicated, with a model, to the Society of Arts, who awarded the silver medal and thirty guineas to the Inventor. See Vol. XXIII. of their Transactions, where the present article is extracted.

† The twist may be seen by holding the piece of the chain by one end, and viewing the links edgeways as it hangs down.

common

common pullies with semicircular grooves, are not in the least calculated to prevent. Hence the alternate links of the chain, in coiling round a barrel, or working over pullies, form obtuse angles in assuming the spiral form, bearing upon the lower parts of their circumferences, and forming as it were two levers, which wrench open and crush each other in proportion to the weight suspended, as well as prevent the freedom of motion in the links themselves, and thereby load the chain with additional friction.

—and its links tend to cut each other, when bearing on a barrel.

A still greater obstruction to the uniformity of its motion, is the tendency which the chain has to make a double coil as it approaches the middle of the barrel and crosses its centre, and that of the pullies at right angles, by means of which the chain is frequently broken by the sudden jerk caused by the upper coil slipping off the undermost.

Its coils will ride upon each other, and jerk when they slide off;

It is to these causes that all the accidents that occur to workmen and machinery from the failure of chains may be attributed (bad iron excepted), and which form the sole objection to their becoming a general substitute for ropes.

—and thus they will break

As a preventive to these evils, I have grooves cast in iron pullies, of sufficient dimensions to receive the lower circumferences of the links of the chain, which work vertically; those which work horizontally and form the gudgeon part of the chain (if we may be allowed the expression), bearing upon each side of the grooves.

The inventor prevents this by grooves in the pullies;

The barrels are also of cast iron, with spiral grooves of the same dimensions, at such distance from each other as to admit the chain to bed without the danger of a double coil; by these means the links are retained at right angles with each other, the only position for free and uniform motion.

—which admit the half of each alternate link.

The links of the chains are made as short as possible, for the purpose of increasing their flexibility, and they are reefed perfectly free from twist, in the pullies, and on the barrels for the same reason.

When applied in blocks, the grooves in the pullies prevent the different falls of the chain from coming in contact, and render plates between them (as in the

—and produce many advantages.

common way) totally unnecessary; the pullies are in consequence brought closer together, the angle of the fall from block to block considerably diminished; and the friction against the plates entirely avoided. Brass guards, with grooves opposite to those in the pullies, are riveted to the blocks, to prevent the chain getting out of its birth from any accidental circumstance. This method of working chains I first put in practice for Messrs. T. W. and B. Botfield, at these works in July last; and it is applied in the working of cranes capable of purchasing from ten to fifteen tons; in the working of the governor balls of steam-engines constructed by Messrs. Boulton and Watt, and in the raising of coal and ore from the mines, for which purposes ropes had before been solely used at this manufactory. In all cases it has performed with the utmost safety, uniformity, and flexibility; so much so that the prejudices of our workmen against chains are entirely done away, and they hoist the heaviest articles with more ease, and as great confidence of safety as they would with the best ropes.

Various successful applications.

The method is easily to be introduced.

The same method is applicable, at a trifling expense, to all machines at present worked by ropes, or by chains, in the usual way: and all the common chains now in use, may be applied to it with equal facility.

Experiments.

With a view of ascertaining the relative flexibility of ropes and chains, I wedged an iron pulley, thirty-one and a half inches in diameter, on the spindle of the pinion of a crane of the following description, viz.

Barrel, 30 inches diameter;
Wheel, 64 teeth;
Pinion, 8 ditto;
Top block, with three pullies of 12 inches diameter;
Bottom block, with 2 ditto, ditto.

A chain applied in this method is much more flexible and easy in its work than a rope.

To the large pulley I attached a small rope, for the purpose of suspending the weights in the hoisting of the different loads, and the results were as follow:

The

The chain in the common way is less so.

The Crane was loaded with	Took to hoist the loads when reefed with the Chain in grooved pullies*.	Ditto, when reefed with a half-worn tarred strand-laid rope, $3\frac{1}{2}$ inches in circumference.	Ditto, when reefed with the Chain promiscuously as in the common way.
lbs.	lbs.	lbs.	lbs.
First . . 2000	63	74	80
Second 1000	32	39	41
Third . . 500	17	21	22
Total . . 3500	112	134	143

The flexibility is inversely as these momenta, and proves the superiority of chains; for (on the average of the trials) with the chain in the grooves,

One pound raised 31,25 lbs.
With a half-worn strand-laid tarred rope, three inches and a half in circumference, ... 26,11 do.
And with the chain in the usual way, only. . . . . . . 24,47 do.

The chain is safer than a rope.

It also appears (contrary to the general opinion), that chains are safer than ropes; for it is an established axiom, that those bodies whose fibres are most in the direction of the strain, are the least liable to be pulled asunder; and in our examination of the properties of a rope, we find that the strands cross the direction of the strain in undulated lines, and consequently prevent its uniform action thereon. A rope is subject to this inconvenience even when stretched in a direct line, but more particularly so when bent over a pulley, as in that position the upper section moving through a greater space than the under one, is acted upon by the whole strain; and hence the frequent breaking of ropes in bending over pullies, from the double strain overloading the strands of which the upper section is formed.

* All the experiments were tried with the same grooved pullies.

 The

The oblique strain from bearing is inconsiderable.

The links of a chain are subject to the transverse strain, where they move in contact; but as such strain is in proportion to the length of the bearing, it must be very trifling. All the links having axles of their own, *the chain moves simultaneously with the strain, and both are in consequence retained in continual equilibrio.* A chain in grooves will therefore sustain as great a weight when bent over a pulley, as it will in a direct line, and consequently is safer than a rope.

Chains are less affected by exposure than ropes.

A safe, uniform, and flexible method of applying chains in the working of machinery, has long been a desideratum in the arts; for they are but little affected by exposure to the weather, or the heat of manufactories, whilst either produces the speedy destruction of ropes.

—and last six times as long.

This discovery is of additional importance, as it substitutes a durable article for a very perishable one, and gives employment to our own manufactories at the expence of foreign importations.—The durability is at least six to one in favour of chains.

The author's crane has valuable improvements.

Though the drawing of the crane is chiefly intended to convey a proper idea of the new method of working chains, yet it will be found to possess several other advantages in point of construction, which are entirely new, and calculated to increase the safety and durability, as well as to lessen the expence of that useful machine.

The mortises of the transverse pieces are commonly distant from the gudgeons.

On reviewing the principles of a crane, we find that the gudgeons are the points of resistance to the machine and its load, and consequently the effect of the transverse strain upon the perpendicular, will be in proportion to the distance of the mortise, for the gib from the upper one; and that of the oblique strain, in proportion to the distance of the mortise for the diagonal stay, from the lower one.

—which requires a strength of timber in the upright-piece.

Notwithstanding these circumstances are so evident, they are seldom attended to; for in general a large and expensive piece of oak, sufficient of itself to make a crane of double the purchase, forms the perpendicular; the gib is mortised into it, at eighteen or twenty inches from the top, to make room for the gudgeon, as is the diagonal stay at five or six feet from the bottom, to allow a birth below for the barrel. Thus the effect of the transverse and

oblique strains of the gib and diagonal stay upon the perpendicular, is increased by their distances from the gudgeons, or points of resistance, and the perpendicular itself considerably weakened by mortises made where the greatest strength is required. Hence the frequent failure of cranes of the common construction, by the breaking of the perpendiculars in the mortises.

Improvement. Cast-iron mortise pieces.

It appears, however, that the various parts of a crane formed of wood, cannot be connected together in any other way than by mortising; and as this method *considerably diminishes the strength of the timber*, I make use of cast-iron mortise pieces.

Method of framing, &c.

The perpendicular is formed of two oak planks, each eighteen inches wide, four thick, and sixteen feet long; these, at the top and bottom, are let into cast-iron mortise pieces, which retain the planks ten inches asunder. The barrel for the chain, works between them. The piece at the top contains in the middle a dove-tailed mortise, into which a stock for the gib is fixed; for greater security, an iron bolt goes through the whole; the stock projects two feet from the mortise, and a plank eighteen inches deep, and four thick, is bolted to each side of it to form the gib, the interstice between the planks forming a birth for the top block to slide in. The diagonal stay is of the same dimensions, formed in a similar manner, and connected to the perpendicular, by being let into the lower mortise piece.

Advantages of this construction detailed.

In this mode of construction scarcely any part of the timber is cut away; and the strength of the materials, so far from being diminished, is augmented by the cast-iron mortise-pieces, the gib is brought much closer to the upper gudgeon, and the centre lines of the perpendicular and the diagonal stay, crossing each other at the top of the lower one, places the whole strain as near as possible in a line with the gudgeons. The business of the perpendicular becomes in consequence little more than that of a mere prop, and consequently requires no greater strength of materials than the diagonal stay.

The top block is made of cast-iron, and has a groove three inches deep on each side, for the purpose of embracing the planks which form the gib.

Lower gudgeon.

To prevent the inconvenience of the dirt of the floor getting into the brass of the lower gudgeon, and thereby obstructing the revolution of the crane, those parts are reverse to the common way, the gudgeon being fixed in the floor, and the socket part which embraces it is cast in the bottom of the mortise-piece, as is also a channel to convey oil to the gudgeon.

Reference to Mr. Gilbert Gilpin's Crane, Plate III. Fig. 1, 2, 3, 4.

Description of the crane by references to the plate.

Fig. 1, Represents the crane with all its parts complete, ready for work.

A B, The perpendicular, formed of two oaken planks, each eighteen inches wide, four thick, and sixteen feet long, let into cast-iron mortise-pieces C D.

E E, The barrel for the chain which works between the two planks of the perpendicular.

F, The top piece, containing in the middle a dovetailed mortise, into which H, a stock for the gib, is fixed; an iron bolt goes through the whole, for greater security. The stock projects two feet from the mortise, and two planks I, K, eighteen inches deep, and four thick, are bolted one on each side of it, to form the gib, the interstices between these planks forming a birth or space for the top block L to slide in. This block is made of cast iron, and has a groove three inches deep on each side.

M, The diagonal stay is of the same dimensions as the gib, formed in a similar manner, and connected to the perpendicular by being let into the lower mortise-piece D.

N, The handle or winch which turns a small pinion O, fixed on the same axis; this pinion works in the teeth of the wheel P, moving on the same axle as the barrel E, on which the chain R lies in spiral grooves.

S, The block and hook by which the goods are raised.

Fig. 2, Is a side view of the handle N, the pinion O the toothed wheel, and the barrel E placed betwixt the two uprights A B.

Fig.

Fig. 3, Shows upon an enlarged scale part of the barrel E, and some of the chain lying in its proper position in one of the spiral grooves, or channels: it is to be noted that the lower edge of one link lies in the groove, and the next link upon the surface of the barrel, and that by this means the chain is prevented from twisting in winding upon the barrel.

Description of the crane by references to the plate.

Fig. 4, Shows a section of part of the barrel E, in order to point out clearly the manner in which one link lies within it, the other link on its outside; it is contrasted by *Fig.* 7, the old method of working chains*.

VII.

Construction of the Anchor and Pallets in Graham's dead beat Escapement. By Mr. J. Bennett.

To Mr. Nicholson.

Sir,

As I have always found the following method of drawing the dead beat escapement for clocks very useful and correct in practice, if you think it deserves a place in your valuable Journal, by inserting it, some assistance will probably be afforded to workmen in that branch of mechanics: and you will oblige

Your's,
J. BENNETT.

Norwich,
6th August, 1806.

Draw the line AB (fig. 4. pl. III.) on which describe the circle B the size of the intended swing wheel: then, according to the number of teeth the pallets are intended to scape over, say, As 60† is to 360, so is double the number intended

Construction of the wheel and pallets for Graham's dead beat.

* Certificates of the highest respectability are mentioned in the Transactions, which were sent to the Society in proof of the advantages derived from sixteen months daily work of chains applied in this method.

† The first proportion must always be double the number of teeth in the swing wheel.

Construction of the wheel and pallets for Graham's dead beat.

tended and one more to that proportion; thus, suppose the number intended to scape over was 9, double of which is 18, to which add one, makes 19; then work it thus:

If 60 gives 360 what will 19

$$
\begin{array}{r}
19 \\
\hline
6,0)684,0 \\
\hline
114
\end{array}
$$

which is the exact space taken up by nine teeth and a half, half of which is 57; then on the circle already drawn set off on each side the line A B, from an exact line of chords, 57 degrees; from which points draw lines to the centre of the circle, then on these points where they intersect the circle, erect perpendiculars, and prolong them till they intersect in the line A, and this intersection is the centre of motion for the pallets, from which centre draw the arch C C, through the circle where it is intersected by the lines from the centre, or those points where the 57 degrees fall; the arch thus drawn is the receiving and leaving pads of the pallets, the inclination, or inclined plane of the pallets to form an angle of 60 degrees with the lines drawn from the centre of the wheel to its circumference; thus, from the point (as a centre) where the arch C C that forms the pallets intersect the circle, draw half a circle D of any size; then for the receiving pallet E, set off from the point *f* an arch of 60 degrees, which will fall on the circle at *g*, then from that point, and the intersection of the arch C C with the circle B, draw a line, which gives the inclination of the pallet E; and for the leaving pallet G, make a similar circle, and from the point *h* set off 60 degrees, which will fall on the circle at *m*, and draw a similar line as before, which gives the inclination of the pallet G.

Investigation

Odrometer for a Carriage
by R L Edgworth Esq

Fig 1

Fig 3

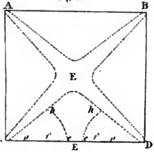


Fig 4

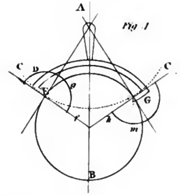


Fig 2

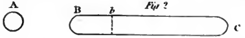
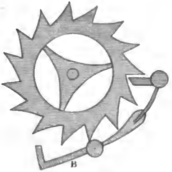


Fig 5



VIII.

Investigation of the Principles which shew that it would be much safer to jump from the Back than from the Side of a Carriage when run away with by unruly Horses. In a Letter from J. E. Conant, *Esq. &c.*

To Mr. Nicholson.

Great Marlborough Street, Sept. 17.

Sir,

As so many accidents continually happen from the destructive expedient of leaping from the side of an open carriage while the horses are running away with it, perhaps you may be induced to insert this in the Philosophical Journal, in which I attempt to determine how far it may be safe in such cases to leap from the *back* of the vehicle. I hope it will be found correct, but I submit it with due deference to yourself and others.

It is more adviseable to jump from the back than from the side of a carriage in motion.

In the following calculation, setting aside the resistance of the air, I suppose a young man (for it is a young practice to put an unruly horse to a carriage) able to spring two feet perpendicularly against the force of gravity; but in falling one second, he, in common with all bodies, would acquire a velocity of 32 feet per second, and have fallen through a space of 16 feet; and the spaces described being as the squares of the velocities, a man in falling 2 feet acquires a velocity of 11 feet per second, and this equals the velocity with which he first springs from the ground.

Estimate of the velocity of jumping.

Suppose the carriage is moving at the rate of 12 miles an hour, and a man springs from the back of it at an angle of about 40° from the horizon with a force as above equal to about 11 feet per second; this force estimated horizontally will be about 9 feet per second, and the effort of the air, so far from resisting, will be in favour of the horizontal projection; this 9 feet per second, or 6 miles per hour, deducted from the 12 miles, leaves 6 miles per hour for the actual horizontal velocity of the man after his leap, which the force of the air will somewhat lessen, and this, with the accelerating force of gravity,

A man can jump with an horizontal velocity of six miles an hour,

— which deducted from that of the carriage will leave six—

gravity, will carry him downward in the projectile curve, and (whatever may be the height of the vehicle) he will come to the ground with nearly the same force as if he had leaped from it while he was standing still, only that he will fall in an opposite direction, and must take care to throw himself in such a position that he does not fall backwards when he touches the ground.

— in the opposite direction.

From the result of this calculation a person run away with in an open carriage may judge how far this mode of escaping will be preferable as to safety.

If a gig be hung low, the leap will probably be easy.

For instance, if a gig is hung so low as to be an easy leap when standing still, he will probably receive little injury by leaping out of it when it is going at the rate of 15, or perhaps even 16, miles an hour, but not more. If he ventures to jump out at the side, the violence with which he would fall to the ground would be almost double, although the height might not be so great.

J. E. CONANT.

IX.

Memoir on Ultramarine, by Messrs. DESORMES and CLEMENT; *read to the Class of Physical and Mathematical Sciences of the Institute, January* 27, 1806 *.

Ultramarine not yet examined.

THE fine blue colour known by the name of ultramarine, has not yet been an object of research to chemists, who have hitherto turned their attention to the lapis lazuli, which may in some sort be considered as its ore, and which has never exhibited itself in a crystalline form, except in a single specimen possessed by Mr. Guyton.

One crystallized specimen of lapis lazuli.

Process of extracting ultramarine.

To extract the ultramarine from its ore, a process is employed, to which art offers nothing analogous, and of the theory of which we are totally ignorant. This process consists in levigating the lapis lazuli, and mixing it well with a melted composition of resin, wax, and linseed oil. When these are thoroughly mixed, the compound is suffered to cool, and then well ground with a pestle,

* Annales de Chimie, Vol. LVII. p. 317.

or

or a roller, in warm water. This water becoming turbid is thrown away, and fresh substituted, which is soon perceived to acquire a fine blue tint. When this is sufficiently loaded with the colouring matter, it is put by to settle, and more water is taken, which likewise assumes a blue colour, but less intense than the former. This is repeated, till the water acquires only a dirty grey hue. From these waters a powder is deposited, which is so much the more beautiful, in proportion as the lapis lazuli was more rich, and according to the order in which the water affording it was employed. The gangue of the ultramarine remains behind in the cement.

The finest ultramarine employed in this analysis.

We employed in our researches ultramarine of various qualities; but that used in the experiments from which we have deduced the approximate proportions of its constituent principles was of the greatest beauty. Only two or three per cent. of this was obtained from a very fine lapis lazuli; yet still it was not perfectly pure, though it was at least fifteen or twenty times as pure as the stone from which it was taken.

Only 2 or 3 per cent. afforded by fine lazulite.

The following are the results of our labours:

Specific gravity.

1. The specific gravity of ultramarine is to that of water as 2360 to 1000.

Action of fire on it.

2. This substance, as afforded by the preceding process, contains oily or resinous matters decomposable by fire: their coal burns completely in contact with air: the ultramarine grows red, and as it cools resumes its former beautiful colour. In this operation it loses a little in quality, and requires levigation to reduce it to the state of fineness and softness it at first possessed.

Fused into a black enamel

3. With a more violent fire, perhaps of 1500° of the centigrade thermometer [2532° Fahr.] the ultramarine fuses into a black enamel, if the cement mixed with it have not been completely burnt away; but, if this have been done, into a transparent and almost colourless glass. In this fusion it loses twelve per cent. of its weight.

— or colourless glass.

With borax;

4. Treated in the fire with borax, it readily gives a very transparent glass; and sulphur is evolved, with a little carbonic acid, the quantity of which varies according to the quality of the ultramarine.

Volta's pile;

5. Exposed to the action of the galvanic pile, the oxigenating

genating end completely deprives it of colour, but the hydrogenating extremity occasions no change.

oxigen gas; 6. Oxigen gas changes the colour of ultramarine exposed to a red heat, causing it to assume a dirty green hue, with an augmentation of weight of one per cent., owing probably to the formation of sulphurous acid which adheres to it.

hidrogen gas; 7. Hidrogen gas in the same circumstances changes the colour of ultramarine completely, imparts to it a reddish colour, and takes from it sulphur. There does not appear to be any water formed, but there is a loss of weight somewhat exceeding that of the sulphur.

sulphur; 8. Sulphur in fusion does not rob it of colour, and after being driven off by volatilization, leaves the ultramarine as beautiful as before.

sulphurated hidrogen; 9. Liquid sulphurated hidrogen has no action upon it.

limewater; 10. Of lime-water the same may be said.

water of barytes; 11. Water of barytes, assisted by heat, deprives it of colour, and is afterward found to contain silex and alumine.

mineral acids; 12. The sulphuric, nitric, muriatic, and oxigenated muriatic acids presently deprive ultramarine of its colour. The first three in a concentrated state form with it a very thick jelly, the fourth dissolves it almost entirely.

If the sulphuric and muriatic acids be diluted with water, sulphurated hidrogen is evolved. The action of nitric acid produces nitrous gas and sulphuric acid.

acetous acid; 13. The acetous acid acts upon it in a similar manner, but more weakly.

alcalis; 14. Potash and soda in solution heated with ultramarine diminish its weight, and are found to contain alumine. They do not alter its colour.

pure potash; If pure potash be heated strongly on ultramarine, its colour is destroyed, the result of the fusion is redish, and comports itself nearly as if the ultramarine were an argil, or a stone composed of silex and alumine.

ammonia; 15. Ammonia has no action upon it.

oil. 16. If oil be heated with ultramine, the weight of the latter is diminished after being washed in an alcaline solution.

Difficult of analysis. 17. The analysis of ultramarine appeared to us more difficult

difficult than that of a stone of an analogous composition would be, though it is very readily attacked both by acids and alkalis. The disunion of its principles is not complete till after the most decided action of each of the reagents employed.

The quality of the ultramarine we used, which we cannot consider as perfectly pure, and the variation that must occur in the proportions of its constituent principles, induced us to study their nature rather than their quantities. To the knowledge of each of these principles we devoted a separate portion of ultramarine; and it is from these results united, that we conclude a hundred parts of ultramarine to be composed nearly of Component parts.

Silex	35	8
Alumine	34	8
Soda	23	2
Sulphur	3	1
Carbonated lime	3	1
	100	0

We always experienced a loss of about five per cent.; sometimes more. Loss.

The carbonated lime we discovered is not essential to the composition of ultramarine, any more than the iron, which we did not meet with in ultramarine of the first quality procured from a lazulite little charged with sulphurated iron. It is not the same with sulphur, which always occurs. Carbonate of lime. Sulphur.

The following is the mode in which we ascertained the nature of the four substances, that appear to us essential to ultramarine. Processes of analyzation.

Thirty grammes [an ounce] of fine ultramine, heated with sulphuric acid, left a residuum weighing 14 grammes. The liquor on evaporation exhibited a few crystals of alum*, and a great deal of sulphate of soda in long needles. Alumine and soda.

* It is probable that the alcali, which occasioned the crystallization of this sulphate of alumine, was potash proceeding from the ultramarine: we do not affirm this, however, because we had not secured the salt from the ammoniacal vapours, that might have existed in the laboratory.

T 2 All

All these crystals and the remaining liquor afforded by means of ammonia 6·85 of dry alumine, and 9·60 of sulphate of soda fused by fire.

These more than the sulphuric acid indicated.

We found by other experiments, that the alumine and soda were commonly in greater quantity than the action of sulphuric acid indicated.

By passing oxigenated muriatic acid gas into water in which 20 grammes of ultramarine were kept in constant agitation, 18·48 were dissolved. The remaining 1·52 had all the characters of silex. From the solution we obtained 4·6 of dry alumine, as much muriate of soda as contained about 4 grammes of alkali, and a portion of sulphate of barytes containing 6 tenths of a gramme of sulphur, supposing it to be composed of 33 per cent. of sulphuric acid, and this acid of 52 per cent. of sulphur. The quantity of silex was not well ascertained.

Silex.

If 5 grammes of ultramarine be fused with 20 grammes of potash, and the compound be treated with alcohol, its weight is diminished one gramme, and the alcohol contains very little silex and alumine. This loss is evidently owing to the soda of the ultramarine, which quits the other principles, because their combination has been broken by the action of the potash in the fire.

On treating ultramarine with carbonated soda, we obtained from 10 grammes 3·3 of silex, possessing all its peculiar characters in a manner less equivocal than was sometimes the case, when it had been procured from ultramarine treated by acids or caustic alkalis. We had supposed, that this was owing to a mixture of some foreign substance, but we were unable to detect any. To ascertain this silex we had employed the ordinary means; and among the rest volatilization by the fluoric acid, which deposited a jelly in the water it was passed through.

Mixture of some foreign substance suspected

Results.

Thus the ultramarine afforded on decomposition silex, alumine, soda, and sulphur.

Theory of the process of extraction.

If we bear in mind, that this valuable substance, as furnished by the process of its extraction, contains oleaginous particles; that soda is one of its constituent principles; that the first waters used for washing away the ultramarine from the cement, with which its ore had been

1 incorporated,

incorporated, are soft to the touch like an alkaline lixivium; and that they leave an alcaline residuum on evaporation; it will be easy to deduce the following theory.

The cement with which the lapis lazuli is mixed is intended to impart oil to the ultramarine, to form a kind of soap, which the warm water carries away, rendering it a little soluble; while the gangue remains united to the cement, in the midst of which it is far from being so easily wetted as the ultramarine, because of its want of soda; in consequence of which it cannot slip like the ultramarine from the fatty, resinous substance, that forms a kind of net for it. In short the process of extraction of ultramarine is a real *saponation* [*savonnage*]; an expression in which we hope we may be indulged on account of its fitness. Theory of ultramarine.

These are the conclusions, that we think may be drawn from our labours, without being too bold. May this first attempt on a substance so little known and so singular be followed by its artificial production.

X.

Abstract of a Memoir on Human Hair; read at the National Institute on the 3rd of March, by M. VAUQUELIN †.*

THE principal objects the author proposed to himself was, to investigate the nature of the animal matter of which hairs are formed, and to ascertain whether there were any thing analogous to it in the animal economy. Object of inquiry; the substance of hair. In the course of his experiments, however, phenomena presented themselves, which, appearing foreign to the principal substance, led him farther than he had intended. It did not originally enter into his plan, to inquire whence the various colours of hair are derived, yet on this subject his attention was most employed. Led by the phenomena to examine the colouring matter. It is only, observes Mr. Vauquelin, by attending a long time to the

* Annales de Chimie, Vol. LVIII. p. 41.

† Messrs. Chevreuil and Caballe, two of Mr. Vauquelin's pupils, assisted him in making the experiments recorded in this Memoir.

same

same object, carefully observing the phenomena that occur, and meditating on the causes that have produced them, that we arrive at results, which it was impossible to have foreseen *a priori*. He does not flatter himself, however, that he has penetrated all the secrets of Nature in this respect, and he offers his ideas with that diffidence which we ought to feel in researches of such difficulty. But he gives an accurate description of his experiments: he compares them, discusses them, and draws from them these conclusions, that appear to him most natural. Of the principal of these experiments, and the corollaries he deduces from them, we shall give a concise account.

Hair boiled long in water

I boiled hairs in water for several days, says Mr. Vauquelin, without being able to dissolvo them; yet the water contained a small quantity of animal matter, as was demonstrated by nut-galls and other reagents.

It is probable, that this matter, which imparts to water the property of becoming putrid, is foreign to the substance of the hair itself. From this experiment I infer, that at the temperature to which water can be raised under the pressure of the atmosphere alone, hair is not soluble in it.

is not dissolved

Dissolved in a close vessel,

I effected their solution, however, without any alteration in Papin's digester, by regulating the degree of heat. In this operation, if the heat be carried beyond a certain point, the hair will be wholly or partly decomposed; as is shewn by the ammonia, carbonic acid, and empyreumatic fetid oil, which is found in the solution, to which the oil imparts a deep yellow colour.

but decomposed in too strong a heat.

Sulphurated hidrogen evolved,

In either case a large quantity of sulphurated hidrogen gas is evolved, which acts strongly on the sides of the copper vessel, turning them black. More is found if the heat be raised to a higher degree, which seems to indicate, that this matter is produced during the operation.

perhaps formed during the process.

Black hair affords a black residuum;

If the hair employed were black, or if the heat were not sufficiently high to decompose the hair, a black matter remains, which falls down very slowly, in consequence of its minute division and the consistence of the solution. This substance is composed chiefly of a black oil, as thick as a bitumen, a little soluble either in alcohol or alcalis, with iron and sulphur perhaps united together.

Red

Red hair leaves a yellowish red residuum, in which are found a great deal of oil, sulphur, and a little iron. red hair a red.

Action of reagents on the solution.

The solutions, when they have been filtered, have scarcely any colour: concentrated acids render them turbid; but an excess of these reagents restore their former transparency, and weak acids produce no change in them. The infusion of galls and oxigenated muriatic acid form in them copious precipitates. Silver is blackened in them, and the acetate of lead is precipitated brown. These solutions, evaporated with all due precaution, did not assume the consistence of jelly on cooling, but remained glutinous; whence I concluded, that the substance of hair is not of a gelatinous nature.

The acids form more copious and deeper coloured precipitates in the solutions of hair effected at a higher temperature, in consequence of their decomposing an ammoniacal soap, which does not take place in the former case.

Solution in weak lixivium.

I have likewise dissolved hair, both black and red, in water containing merely 4 per cent. of caustic potash. During this solution hidrosulphure of ammonia is evolved, which seems to announce an incipient decomposition of the black hair, leaving a black residuum composed of a thick oil, still a little animalized, with iron and sulphur. After the solution of the red hair, a yellow oil containing sulphur and a few atoms of iron remains.

Action of acids on it.

In these solutions acids form white precipitates, soluble in an excess of the acids. When these precipitates are thus redissolved in acids, at the expiration of a certain time an oil appears on the surface in the form of a pellicle with the prismatic colours.

Of lead.

The solution of hair in potash precipitates lead of a black hue on account of the hidrosulphuret it contains. That of the red hair appears to contain most. When it has been freed from sulphur by exposure to the air, it has only a smell of soapsuds, like which it becomes mouldy.

Effects of the different acids.

Each of the acids acts in a particular manner on hair. The sulphuric and muriatic acids assume at first a fine rose colour, and afterward dissolve the hair. The nitric acid turns hair yellow, and likewise dissolves it by means of

of a gentle heat. The solution exhibits on its surface a black oil, if the hair were of that colour, and a red oil if the hair were red. Both these oils ultimately grow white, and become concrete on cooling.

Yield oxalis acid, bitter substance, iron, and sulphur.

This same solution, properly evaporated, affords much oxalic acid; and the uncrystallizable mother water contains the bitter substance, a great deal of iron, and sulphuric acid arising from the sulphur of the hair.

In red hair less iron, more sulphur.

The solution of red hair in nitric acid contains less iron, but more sulphuric acid, than that of black hair.

Oxigenated muriatic acid gas.

Oxigenated muriatic acid gas at first whitens hair, soon after softens it, and reduces it to the form of a viscous and transparent paste like turpentine. This is bitter and partly soluble in water, partly in alcohol.

Destructive distillation.

From hair subjected to the action of fire in a close apparatus I have obtained the same products as from any other animal substance, with this difference, that it furnishes more sulphur, and gives out very little gas. It leaves in the retort twenty-eight or thirty hundredth part of coal.

Incineration.

By incineration they yielded iron and manganese, which impart a brown yellow colour to the ashes; phosphate, sulphate, and carbonate of lime; a little muriate of soda; and a considerable portion of silex. The ashes of red hair are less coloured, because they contain less iron and manganese; those of white hair likewise contain less, but we find in them a great deal of magnesia, at least a great deal with respect to the other principles, for hair scarcely yields above ·015 of ashes.

Alcohol extracts two oils from black hair;—

Alcohol extracts from black hair two kinds of oil: the one white, which on cooling subsides in the form of little shining scales, the other, which separates as the alcohol evaporates, is of a greyish green hue, and ultimately becomes concrete also.

and from red but different.

Red hair likewise affords a white concrete oil like spermaceti, but the alcohol on evaporation lets fall another oil as red as blood. What is remarkable and interesting in this experiment is, that the reddest hair subjected to it becomes of a deep brown or chesnut colour; whence I conclude, that the colour of red hair is owing to the presence of this oil.

From

From the experiments related in the Memoir of Mr. Vauquelin, a great number of which, being merely accessory to the principal object, we have omitted, it appears, that black hair is formed of nine different substances, namely:

1 An animal matter, which constitutes the greater part; Constituent parts of black hair.

2. A white concrete oil in small quantity:

3. Another oil of a greyish green colour, more abundant than the former;

4. Iron, the state of which in the hair is uncertain;

5. A few particles of oxide of manganese;

6. Phosphate of lime;

7. Carbonate of lime in very small quantity;

8. Silex, in a conspicuous quantity;

9. Lastly, a considerable quantity of sulphur.

The same experiments shew, that red hair differs from black only in containing a red oil instead of a blackish green oil: and that white hair differs from both these only in the oil being nearly colourless, and in containing phosphate of magnesia, which is not found in them. Difference of red hair, and of white.

From this knowledge of the nature of the constituent principles of hair Mr. Vauquelin thinks we may account for the various colours that distinguish it. According to him the black colour will be owing to a black and as it were bituminous oil, and perhaps likewise to a combination of sulphur with iron. Carotty and flaxen hair will be occasioned by the presence of a red or yellow oil, which when deepest, and mixed with a small quantity of brown oil, produces the dark red hair. Lastly, white hair is owing to the absence of the black oil and sulphurated iron. He believes, that in the carrotty and flaxen, as well as in the white, there is always an excess of sulphur; since, on the application of white metallic oxides to them, such as those of mercury, lead, bismuth, &c., they grow black very speedily. The manner in which this substance acts on metallic bodies leads him to suspect, that it is combined with hidrogen. Causes of the difference of colour in hair; Blackened by metallic oxides.

Mr. Vauquelin attempts next to explain the whiteness produced suddenly in the hair of persons struck with profound grief or great terror. To explain this, he says, Whiteness suddenly produced

we must suppose, that in these critical moments, when nature undergoes a revolution, and the natural functions are in consequence suspended or changed, some agent is developed in the animal economy, which, passing into the hair, decomposes the colouring matter. But what agent can produce this effect? The acids alone appear capable of it: at least this is certain, black hair immersed some time in them, particularly in the oxigenated muriatic acid, whitens very evidently.

owing to an acid.

Arguments in defence of this opinion.

The rapid production of an acid in the animal economy does not appear to him impossible, considering that a fit of anger in men, as well as in inferior animals, is sufficient to change the nature of certain of their fluids, and render them venomous; and seeing that the galvanic fluid frequently occasions the formation of an acid or an alkali, according to circumstances, both in animal and vegetable substances. As to the whiteness produced gradually by age, he ascribes this to a deficient secretion of colouring matter.

Whiteness from age.

Properties owing to the colouring matter.

In hair, exclusive of the animal matter that forms its basis, and which is the same in all, there is a colouring matter, that may be separated from it, and the hue of which varies according to the kind of hair of which it constitutes the distinction. To this fatty substance Mr. Vauquelin attributes the suppleness, elasticity, and unalterability, which exist in hair: to this substance too is owing no doubt the property it has of burning so rapidly, and that of forming soap abundantly with alkalis.

Its animal substance

After having treated of the colouring matters of hair, he endeavours to characterise the animal matter, that forms its substance, by comparing it with all those of which we have any knowledge. Without relating all the experiments he has made in this respect, we shall observe, that it is not gelatine, since its solution in water, which is difficult to effect, never becomes a jelly on evaporation; neither is it albumen, for this would not dissolve in boiling water without being decomposed, and its solution would be differently affected by reagents.

is neither gelatine,

nor albumen;

but mucus, or something similar to it.

The humour to which the substance of hair approaches nearest, if it be not absolutely the same, is, according to Mr. Vauquelin, that which physiologists have designated by

by the name of *mucus*, or animal mucilage, which is neither gelatine nor albumen.

This humour which is secreted in the nostrils, mouth, æsophagus, trachea, stomach, bladder, and all the cavities of the body in general, imparts to water considerable viscosity, and the property of frothing greatly on agitation. In certain species of coryza it may be drawn out into threads like the substance of silk or the web of the spider; retains its transparency and flexibility after desication; and Mr. Vauquelin has no doubt, that it would perfectly resemble hair, if it contained a little oil.

Mucus.

In some diseased states approaches to hair.

The epidermis, nails, horns, wool, and hair of beasts in general, are formed of the same animal mucus, and equally include in their composition a certain quantity of oil, which imparts to them the suppleness and elasticity they are known to possess.

Other parts formed from it.

Mr. Vauquelin has begun an examination of the humour of the *plica polonica*, with which he was furnished by Mr. Alibert, physician to St. Lewis's Hospital; and from what he has done, he is led to believe, that it is of the same nature as the substance of the hair, but secreted in greater quantity than the formation of the hair requires.

Humour of the plica, a superfluous secretion of the matter of hair.

XI.

Abstract of a Memoir on a new Principle in Meteoric Stones: by A. Laugier.*

EVER since the English chemist Mr. Howard, called the attention of philosophers and naturalists towards the stones called meteoric, all chemists who have repeated the experiments laid down in his interesting memoir, have obtained similar results. They all agree that whatever the time, or wherever the place in which these stones have fallen, their component principles have been the same, viz. silex, iron, manganese, sulphur, nickel, with a few accidental traces of lime and alumine. We see, in comparing the results of their analysis, that these principles exist in very nearly equal proportions. M. Proust has lately

Composition of meteoric stones

* Annales de Chimie, Vol. LVIII p. 261.—June 1806.

announced the existence of manganese in the meteoric stones analyzed by him, which fact is confirmed by those chemists who have since bestowed their attention on this subject.

They contain chrome.

M. Laugier, professor in the pharmaceutical school of Paris, assistant naturalist and operator of analysis to the Museum of Natural History, in analysing a meteoric stone which fell at Verona in 1663, has discovered a principle hitherto unobserved in stones of this kind. This principle is *chrome*, and is the subject of the present memoir.

The author discovered it by using alkali as his first agent.

"It is very probable," says the author, "that I should have overlooked the presence of Chrome, had I not deviated from the method of analysis usually adopted. Acids have always been made use of, which are perhaps the most natural and commodious agents; but in this case I employed caustic alkali, which has the advantage of indicating the presence of chrome, however small may be its quantity, whilst it remains almost imperceptible when held in solution by acids, particularly if blended with any quantity of iron, manganese, &c."

Process. Solution in caustic alkali; washing supersat. with nitric acid. Precipitation of the chrome by nitrate of mercury.

The following is the author's mode of separating the chrome, and of determining its proportion: He dissolved one part of the stone in three parts of caustic alkali, and washed the mass with distilled water, which received a yellow colour, or a greenish-yellow from the manganese: on leaving the mixture to settle, the manganese fell to the bottom, and the liquor regained its pure yellow colour. The solution was then re-mixed with the washing, and after being sufficiently diluted with water, to prevent the precipitation of the silex, super-saturated with a slight excess of nitric acid. Recently prepared nitrate of mercury, at the minimum, was poured into this solution, which immediately threw down a red orange-coloured precipitate, or chromate of mercury; this was suffered to remain till the next day to subside, when the supernatant liquor was decanted, and the precipitate washed in several waters, until deprived of all taste; it was then thrown into a platina crucible, the water evaporated, and the chromate of mercury by desication decomposed into green oxide of chrome, whose quantity amounted to about a *hundredth* part

part of the weight of the stone operated upon. This green residuum presents all the qualities of oxide of chrome.

Other stones of this kind contain chrome.

As the stone of Verona is similar in all its physical properties to other meteoric stones, the author of this memoir wished to ascertain if others also contained the chrome found in that of Verona. He accordingly examined fragments of the stones which fell at Ensisheim, at l'Aigle, at Barbotain, near Bordeaux, and recently near Apt; in each of these four stones he discovered the presence of chrome. It is remarkable that the stone of Verona, wherein he first observed this metal, is of all the five, that which contains the least, its portion being only half a centenary, whereas the others contain a full centenary.

General conclusions.

The author draws from his memoir the following conclusions, in which he is fully countenanced by M. Vauquelin.

1. That the five meteoric stones of Verona, Barbotain, Ensisheim, Aigle and Apt, contain, besides those principles already known to chemists, the metal called *chrome*, in the proportion of about one hundredth part.

2. That it is very probable that all meteoric stones possess this principle; since they all resemble each other in their physical and chemical characters, and have all, so far as has been hitherto ascertained, the same origin.

3. That in many cases, in order to attain the requisite precision of chemical analysis, it may perhaps be expedient to treat the same substance with both acids and alkalis; as experience demonstrates that parinciple may be overlooked in one case, which will be obvious in the other.

XII.

Account of the dreadful Fall of the Summit and Part of Mount Rosenberg, which happened on the 2nd of last Month.

Bern, Sept. 7.

INFORMATION has lately been received of a dreadful accident which has destroyed several villages in the canton of Schwitz, situate between the lakes of Zug and

and Lauwertz. M. M. Freudenreich and Schlatter, directors of the mines, set out yesterday evening, by order of Government, to give aid. The following are the details of this disaster, the most dreadful recorded in the annals of Switzerland:

Sudden fall of the summit of mount Rosenberg, overwhelming five villages and partly filling a lake.

"On Tuesday, the 2d of September, at five in the evening, the Knippenbuhl Rock, which formed the summit of Mount Rosenberg, was on a sudden detached from its station, and at the same time part of the mountain, of several feet in thickness, on the western side, and about 280 feet in thickness on the east side, gave way, and fell into the valley which separates the lake of Zug from that of Lauwertz, overwhelming the whole of the villages of Goldau, Rœthan, Busingen, Huzloch, three parts of that of Lauwertz, and some houses in the village of Stein. The fall of one part of the mountain into the lake of Lauwertz, about a fourth part of which is filled up, caused such an agitation in the waters of the lake, that they overthrew a number of houses, chapels, mills, &c. along the southern shore of the lake; amongst others, the mill of Lauwertz where fifteen persons were killed and buried in the ruins of the buildings, all the parts of which were dispersed with such violence, that the foundation only remains. This mill was situated 50 or 60 feet above the level of the lake.

Sufferers.

"The waves also beat against the village of Seeven, situate at the extremity of the lake, and destroyed some houses. Two persons were killed.—In the villages which were overwhelmed, not an individual escaped. Upwards of 1,000 persons have been victims of this disaster. A society of travellers, thirteen in number, were on the road from Arth to Schwitz: nine, who walked first, perished; the other four, who were about forty paces distant, escaped. Those who were killed, were, M. M. Rodolph Jenner, of Brestenberg; Colonel Victor Steigner, of Berne; Charles May, of Rath; Doctor Ludwig, of Arbon, in Thurgovia; Mademoiselle Diesbach, of Berthoud; Madame Diesbach, of Watteville; Madame Frankhauser, of Berthoud; and two guides, of Arth. Five minutes sufficed to complete this disaster.

The effects ex-

"At Schwitz, some persons heard the noise, and saw at

at a distance the vapour which covered the place where the accident happened, and which was carried towards Sulug, on the opposite side, with a strong sulphureous smell. The falling of the mountain extended from the summit to the opposite side, beyound the Lake, a distance of three leagues from north to south, and a league and a quarter from west to east. There is nothing now to be seen but melancholy ruins, through the whole of that country, which presented the richest communes in the canton of chwitz, inhabited by a brave and faithful people. Only thirty persons remain out of this interesting population.

tended three leagues. Sulphureous smell

" Several circumstances attending this event are very remarkable. Enormous masses of rock were carried through the air to prodigious distances. The rocks in falling, drew with them immense masses of earth, of from ten to eighty feet in thickness; and numbers of these masses, together with large blocks of flint stone, were thrown on the opposite shore, to the height of from eighty to one hundred feet. One can scarcely believe ones eyes when one seas these *phenomena*. Every instant one sees houses, some forced on one side, others cut in two, and separated at great distances; and others carried more than a quarter of a league from their foundations.

Massesofrocks projected; or rebounding.

" The Lake of Lauwertz has lost about a quarter of its extent, but its recovered part is filled at present by the waters of several brooks, which no longer flow. That rich plain, which was so beautiful, now presents a mountain of near 100 feet in height, of a league and a half in length, and as much in breadth.

Effect on the lake.

" Mount Rosenberg bears E. N. E. from Arth. It is its western part which has fallen down; that which was on the side of Arth, after descending direct towards its base, was suddenly thrown to the east, and thus Arth, Zug, and all that side of the Lake were saved. The thickness of the mass carried down, appeared to be two feet on the western side, and upwards of 150 on the east side. The Knippenbuhl seemed to have announced this misfortune so early as the year 1774, when it detached itself from the mass of the mountain. The Isle of Schwanau, elevated on a rock, in the middle of the Lake experienced also some damage, particularly its church.

Account of the mountain.

The

The good Hermit was fortunately at Ensiddlen. The long road of the Lake is broken in a thousand places.

Farther particulars.

"Succours have been sent with the greatest promptitude. Six hundred workmen from Zug and Schwitz have gone to the banks of the Lake of Lauwertz, particularly the mouth of the Seven. This small river was so obstructed by ruins of all descriptions, wood, trees, houses, &c. that, without prompt assistance, the safety of all the houses below Schwitz to Brunnen, would have been menaced.

"One man had the good fortune to withdraw in time under ground, with his servant and a child, which he held in his arms. In one house near Arth is still living, a poor man, who had both his thighs broken. During the search which has already been made, twenty persons were discovered dead at the entrance of the village of Goldau, men, women, and children, some having their arms, others their heads, others their legs separated from their bodies, and the bodies of some cut in half. We have coasted along the foot of Riga, where the greatest part of those who survived this catastrophe took refuge: alas! not more than thirty. An old man whom we met, said to us, "I had sons, daughters, and a great number of grand children. I had a wife and other relations. I alone remain." A little girl said, "I have no longer father or mother, brothers or sisters." A woman had lost her mother, husband, brothers, sisters, and five children.

"The villages of Goldau and Rothen, consisting of 115 houses, that of Busingen, of 126, and that of Huzloch, have totally disappeared. Of Lauwertz, which lost 25 houses, there remains ten buildings, all much damaged. Stein has lost two houses and several stables, which were in great numbers in all these villages.

"P. S. Twenty years since, General Psyffer predicted this catastrophe, from the knowledge which he had of the mountain. A professor of Schwitz said, that above Spietzflue was a sea of water, which had undermined the rock for several years, and that below there was a cavern of great depth, where the waters were engulphed. The quantity of water which has fallen during the preceding years, has hastened this catastrophe, and the rains of some weeks past have decided,"

XIII.

Farther Remarks and Experiments on Vision under Water. In a second Letter from a Correspondent: with some Observations in Reply. By the Editor.

London Institution, Sept. 17, 1806.

To Mr. Nicholson.

Sir,

I HAVE to thank you, (before I make any further remarks on the Paper in your 58th number, "On the Art of Swimming,") for the insertion of my letter in your 60th Journal, and for the candid manner in which you were willing to investigate, whether my objection to what you therein stated was founded on fact. Introduction.

I have in consequence of your reply been induced to make some actual experiments, since I had the pleasure of reading the account of those, which, together with your two friends you had made. I will presently state them to you, and they will, I think, clear your mind of every doubt that can be entertained on the subject; permit me however first to revert to the Paper in which you differ in opinion with Dr. Franklin—your words are these, "I am rather surprised at the Doctor's direction about the egg, and the eyes open under water, because it seems as if he thought the submerged experimentalist could see the egg." You after that say you must refuse your credit to such assertions, and add that "Experiment will easily clear up the matter to those who know nothing of optics." You then state your unsuccessful attempt to pick up your buckle in five feet water at Joanna, your experiment at Harlem follows, and your conclusion is thus drawn in your own words. "Whence it appears, that all the stories of wonderful divers who could descend into the sea and bring up small objects, such as jewels and trinkets must be considered as fabulous." In the first writing of W. N. he treated the subject as if it were impossible to see under water.

On your reply to my letter, I have to remark, that the cylindrical glass vessel, with which you made your experiments Objections to the Experiments of W. N.

periments is almost as objectionable as the washing tub, or wash-hand bason. I would ask if the very form of your glass vessel might not have the effects of refracting the rays of light, or of their crossing so as to cause an imperfect vision of the object looked at? You of course know that in our Courts of Judicature, the evidence of a person swearing to a transaction seen through blown glass is not held admissible; and here I do not see, what occasion there is, in making the experiment, that the rays of light should pass through any other medium but the water itself. Now with respect to the distance, the focus was obtained under water, it appears to me not to be the question, but whether the object was visible or not. It so happens I am short sighted, but it surely cannot be contended that I cannot see, because I hold a newspaper nearer my eye than another, as the eyes of different people of the same age are of different focus, but they still see, perhaps equally well at their respective foci. Age also will cause a difference. I have no doubt that convex lenses would enable the eye to see clearer at a greater distance in water, for we know the crystalline humour in fish is almost globular, and doubtless for that purpose.

In your "conclusion that the human eye cannot distinguish objects under water," I think you begin to discover that your former opinion *may* be fallacious, as you are disposed to question whether some people may not see *imperfectly* under water, and conclude thus; "but I must confess that I do not incline to that opinion."

New experiments, in the Thames. A diver brought up various objects from a depth of near nine feet.

The experiments I made are these: I went on the Thames in a boat a little above Richmond, accompanied by two friends; we took with us a native of Africa who was to make the experiment before us; we took in the boat two eggs, thinking them the most proper for trial, from what you had stated in the 4th paragraph of your reply; one of which I had spotted with red sealing wax. I first threw the spotted egg into water, between eight and nine feet deep, where the water was not very clear; for we could not see the egg at the bottom from the boat: our diver on the first attempt readily brought us up the egg. I then threw in both, desiring him to bring up the plain one only, which as readily he did, though he saw the

the spotted one at the bottom close to it. I afterwards threw in the white and a piece of painted wood, 4 inches long, prepared for the purpose, with lead in it to sink it. He then brought up the egg in one hand and the wood in the other. Being perfectly satisfied with these experiments, and the wind being at North East, we did not desire our diver to repeat them.

For my own part, I confess, I should not like to contract with our African friend to throw in as many guineas separately, as he would separately pick up in the same depth of water, provided the bottom was free from weeds and mud, though he were to give me two for each he missed finding.

—and probably would have succeeded in all his attempts.

Perhaps the relation of these facts may strike your mind less forcibly,

Proposal to repeat these experiments.

> Quàm quæ sunt oculis subjecta fidelibus, et quæ
> Ipse sibi tradit spectator.

If so I shall be very happy to have these on any other experiments, more convincing if possible, exhibited before you and your friends whenever you may please to appoint.

I could cite many accounts of the pearl fisheries from respectable authors; and the fact of divers bringing up the particular shell fish, that produce the pearls, I think stands upon incontrovertible evidence, though you have, misled by a false hypothesis, treated these accounts as fabulous.

Narratives of divers.

I am well convinced, Sir, from your known character, that if you find you have been mistaken in what you have asserted, you will be most ready to allow it, thereby verifying what Solomon long since has said. "Give opportunity to a wise man and he will yet be wiser."

Conclusion.

I am,
Sir,
With the greatest respect,
Your most obedient servant,
A DIVER.

Reply. W. N.

Question whether W. N. or his correspondent have reasoned correctly from their facts.

After expressing my satisfaction that the present subject of enquiry has been treated by an immediate reference to facts, I will take the liberty to make a few remarks, chiefly with a view to indicate what conclusions we ought to deduce from them.

W. N. because he cannot himself see under water, has concluded that no one can. The diver has made the opposite conclusion from facts also limited to some men only.

I admit that every remark I made upon Dr. Franklin's assumption, that men can see under water, was founded upon my own experience, that the contrary position, with regard to myself, is true; and my error appears to have been of the same nature as that of my able oponent.—I, by making a general inference from particular facts, have concluded that *no man can see or distinguish under water*;—he, on the contrary, making too extended a deduction from his own observations, has concluded that I have been misled by a false hypothesis, and seems to think that *all men can see under water.*

I think the present controversy has given us both sufficient reason, to enquire whether, among men who can see objects in the air at all distances, with considerable distinctness, there be not many who like myself, cannot distinguish at all under water; and many others, who like my correspondent, can see almost as distinctly in that element as they do out of it. At all events the former cannot be a question of any doubt to those who find they do not distinguish (or see) under water.

Vindication of the use of the glass vessel.

It does not seem to me necessary to enter upon any discussion respecting my cylindrical glass, because my experiment appears to have been misapprehended. There was nothing but water between the eyes and the objects, and yet we could not distinguish them. The vessel being glass, the objects were well enlightened; in effecting which the figure of the glass was of no consequence, as we did not look through it. The experiment of the lens neither confirms nor weakens the truth of the general fact.

Whether the eye can alter its figure so as to see in air at two inches distance. Probably not. --But this seems

When I was a young man, I saw objects distinctly at four inches distance. At present I see imperfectly at less distances than twelve inches, though I am still near-sighted with regard to objects more remote than four feet. As the curvature and refractive density of the human cor-

nea

nea may differ in different subjects, and have not, that I know of, been well determined, I shall not attempt to treat this inquiry on the strict principles of optics; but only observe that I was then able to do by adjustment of the organ, what would now require the assistance of a four inch lens; and that I do not apprehend that I could then, or that any one else can, see distinctly in the air at two inches distance. But this appears necessary to be done by an eye like mine in order to see under water.

wanting for distinct vision under water.

A certain part of the refraction performed by the eye is effected by the anterior surface of the cornea, and the rest by the internal structure. Two eyes may be so constructed, as that in one of them the cornea may perform a very considerable part, and in the other, the greater part may be effected by the internal structure; and both these eyes may see equally well: but they would not both be alike affected by immersion in water. That with the most prominent cornea would afford the least distinctness; the other might be so little affected as to give distinct vision under water, by a change within the reach of its ordinary adjustment. Do the curvatures of the cornea in different subjects, who see well, vary sufficiently to admit of this solution of our difficulty?—This must be referred to experiment.

The flatter the cornea the less would an eye be affected by immersion in water.

Are human eyes so various?

My correspondent has offered to exhibit his experiments before me. I shall be happy to accept his kindness, and at the same time should be glad if his African friend would look into my large jar.

Offers to repeat the experiments.

XIV.

Experiments and Observations on the Adhesion of the Particles of Water to each other. By BENJAMIN, *Count of Rumford, F. R. S. &c Communicated by the Author to the National Institution of France, and transmitted to him by the Editor.*

(Continued from page 56.)

IF the particles of water adhere strongly to each other, it appears to me to be a necessary consequence that a kind of pellicle will be formed at the surface of the liquid, and even at all these surfaces, whatever may be in other

The film is considered as the consequence of the adhesion of the aqueous particles.

other respects the mobility of these particles, or rather of the small liquid masses composed of a great number of them, when they are remote from the surface and possess their fluidity without impediment.

Film at the lower surface of the water.

When a small solid body, placed on the surface of water becomes wetted, it immediately descends beneath the pellicle, which no longer opposes its subsistence. At this period the viscidity of the water begins to manifest itself in a very different manner, but with infinitely less effect than when it acts at the confines of the liquid. But it is not yet time to enquire into this part of our subject.

Water was poured on mercury and ether upon the water.

With a view to render sensible the resistance which the pellicle of the inferior surface of a stratum of water opposes to a solid body which passes through that stratum by falling freely downwards, I made the following experiment.

EXPERIMENT VI.

The lower surface of the water next the mercury supported a larger globule than the upper could have done.

Having filled a small wine glass to about half its height, with very pure mercury, I poured a stratum of water of three lines in thickness upon the mercury, and upon that a stratum of ether of two lines.

When the whole was at rest, I took with the small tool before described a spherule of mercury of about one third of a line in diameter, and let it fall through the stratum of ether.

This spherule being too heavy to be supported by the pellicle at the superior surface of the water broke it, and descended through that fluid; but upon its arrival at the inferior surface it was stopped, and remained there preserving its spherical form.

I moved this spherule with the extremity of a feather, and even compressed it; but it always preserved its form without mixing with the mass of mercury on which it appeared to rest.

The lower surface of gum water supported a still larger globule.

It was no doubt the pellicle of the inferior surface of the stratum of water which prevented this contact, and as this pellicle was supported by the mercury on which it rested, I was not at all surprised to find that it could support without being broken, a spherule of mercury much larger than the pellicle of the superior surface could support.

In order to satisfy myself that the viscidity of the water was

was the cause of the suspension of this mercurial globule at the bottom of that fluid I repeated the experiments and varied it by substituting water, containing a certain quantity of gum arabic, in solution, in the place of pure water; and I found in fact that much larger spherules were supported when the viscidity of the water was thus augmented.

[To be concluded.]

XV.

*Observations on some Errors contained in Mr. Thomas Reid's Paper, published in the Journal of the last Month. From a Correspondent *.*

As Mr. Reid's paper inserted in the last Number of Mr. Nicholson's Journal, contains some statements which materially affect the history of chronometry, but which, on referring to the original documents appear to be very incorrect, it seems proper not to lose time in rectifying errors which otherwise might mislead such persons as have no opportunity or leisure to investigate the matter in question. Introduction.

The invention of the detached escapement is one of the points the most satisfactorily ascertained in the progress of time-piece making. It is clearly due to P. le Roy, who first produced an escapement of that sort, and presented it in 1748 to the Academy of Sciences at Paris; and his claim has never yet been disputed with any degree of reason, though considerable discussions have arisen respecting the pretensions of other artists who since the above date have thought proper to aspire to a share in the same honour. The subject has been canvassed on several occasions, during the long interval of time which has elapsed since that epoch; and every thing that could be said upon it seemed exhausted, till the present moment, when Mr. Reid, in a postscript to his paper, tells us, that Mr. Thiout in 1741 † published the description of a

The invention of the detached escapement belongs to P. le Roy.

Mr. Reid asserts that Thiout invented it before.

* Viz. the author of the Memoir at page 273 of our last Vol.

† The title of Thiout's work is, "Traité de l'Horlogerie méchanique et pratique, approuvée par l'Academie Royale des Sciences." 2 vol. 4to.

 detached

detached escapement, which, if it were really such, woul of course be the first model of that idea ever commun cated to the world, and would consequently degrade I Roy from the rank of an inventor to that of a mei copyist or imitator. The real state of the case, howeve is as follows:—The Academy of Sciences in 1748 receive the escapement of Le Roy as new, though Thiout's bool was published only a few years before; and no oppositio was made to this declaration by the author, (whom w believe to have been then alive,) or by any of his friend and successors. One of the committee who examine that invention was Mr. le Camus, a good judge of watch making, who, as such, came, afterwards, with Mr. Ber thoud, to this country, by order of the French Govern ment, in order to witness the disclosure of the principl of Mr. Harrison's time-keeper; and the same Mr. le Ca mus, who first mentioned that Dutertre the elder ha conceived the idea of a detached escapement, according t a construction which was preserved in his family, neve said any thing respecting the invention of Thiout, al though that invention was described in a work generall known. Nay, the Academy of Sciences continued s miserably blind, that when, some years after, Mr. Pla tier submitted to that body a new detent escapement their committee declared, that P. le Roy was the first wh had thought of this sort of escapement*. The sam ignorance, or wilful error, has, since that time, attende not only the philosophers who have candidly endeavoure to investigate these matters, but also the critics and rival who may have maliciously searched for circumstance likely to humiliate the pride of the different competitor who have pretended to the credit of originality in th construction of the free escapement. This we canno but observe seems an extraordinary case. The trutl however is, as may be easily guessed from the precedin statement, that the representation of the escapement, a published by Mr. Reid, is totally different from th

His contemporaries never thought so;

— nor subsequent writers.

Mr. Reid has altered the escapement from that published by Thiout.

* "M. le Roy l'ainé est le premier qui ait pensé à cette sort "d'échappement" Observations sur la Physique, &c. pa M. l'Abbé Rozier, T. III. Part I. Juin 1774.

figur

figure given in Thiout's work. Mr. Reid saves himself the trouble of referring to the passages of the books he quotes in the course of his paper; but, as our object is, not only to rectify, but to enable other persons to judge for themselves, we think it incumbent upon us to do it for him; and shall also annex a faithful copy of that part of the abovementioned work, from which he has taken the escapement here treated of, but which he has altered into a detached form.

— and has not quoted his original.

Fig. 5. Plate 3. is a copy of the representation given by Thiout (Fig. 30. Plate 43. Vol. I.), and the following is a literal translation of his explanation of that mechanism (p. 110. ditto): "It is an escapement of a watch in which half of the vibrations *seem* independent of the train of wheels, while they are performed. The detent B stops the escape wheel; the balance bringing back the pallet A, the detent recedes, to leave the escape wheel free to strike the pallet; and so on. This escapement could not perform without a spiral spring*."

Correct copy and translation here given.

From the preceding description, it plainly appears that this escapement acts with a single pallet, and a detent to stop the train of wheels during the intervals that elapse between the successive communications of the maintaining power; but that the balance in it, is never disengaged from the detent; consequently this construction does not possess the distinguishing principle of the detached escapement. Mr. Reid has made it a very different thing, by introducing certain alterations, which will be easily perceived, on comparing his figure with that of Thiout. He has broken the communication between the detent and the pallet, and provided the former with a fork, which is not in the original; and the escapement, with these changes, seems, at first sight, capable of acting freely. Such, however, is not the construction given by

Explanation. Thiout's escapement.

The detent moves with the balance during the repose of the wheel.

Mr. Reid's altered escapement seems to be free;

* For the reader's satisfaction, as well as our own, we subjoin an exact copy of Thiout's explanation: " Fig. 30. est un échapement de montre dont la moitié des vibrations *paroissent indépendantes* du rouage, pendant qu'elles se font. Le crochet B retient le rochet; le balancier ramenant la palette A, le crochet s'éloigne pour laisser le rochet libre à frapper la palette; et ainsi de suite. Cette sorte d'échappement ne sçauroit aller sans spiral." P. 110.

 Thiout;

—but it is not Thiout's, and is incomplete, as a free escapement.

Thiout; and it may be farther observed, that the sort of mechanism represented by Mr. Reid is incomplete, and could never have been executed in that form, there being no provision to keep the detent, by a spring or otherwise, in its proper place, and prevent its getting disengaged in consequence of external motion.

Thiout himself did not set much value on this contrivance.

Thiout was so far from attributing to his escapement the merit which, after making it suffer a complete metamorphosis, is now claimed in its favour, that, in a section of his work where he gives very particular rules for the construction of many escapements, he rests satisfied, respecting this, with the short explanation we have already copied. With regard to the property in question, he says, not that it is, but that it *seems* detached; and this expression is, if possible, more conclusive, when the articles of the same chapter are read in their natural order. The passage above extracted comes after the description of an escapement (pp. 105 and 106) for pendulum clocks, acting with a single pallet and a detent, which was invented by Sully, and formerly published in the *Règle Artificielle du Temps*. Thiout's escapement is an

He made it in imitation of one of Sully.

imitation of the above mechanism, adapted to watches with a balance; and the opinion he entertained of the original, is positively expressed in the following words of his description: "It seems that half of the vibrations are independent of the train of wheels; but this is what experience does not prove*."

Mr. Reid supposed to be partial to the escapement he has drawn.

There are no bounds to Mr. Reid's predilections in favour of Thiout's escapement, and he reminds us of a lover making the picture of his mistress, who cannot draw a feature without embellishing it, and, after all, produces a figure from which no one can form the least idea of the original. Not satisfied with placing the mechanism in question at the head of that most honourable branch of

By making the pallet curved, though straight in the original, he considers it as the origin of the virgule escapement.

the family of escapements called detached; he represents the pallet under such a form as authorises him to conclude that Mr. Thiout's escapement is also the origin of the *échapement à virgule*. But Mr. Thiout has as little

* "Il paroit que la moitié des vibrations sont indépendantes du rouage; mais c'est ce que l'expérience ne prouve point." P. 106.

right to be esteemed the father of the last mentioned escapement, as of the detached. From the copy we have given of his figure, it will be immediately perceived, that the pallet of the original escapement is strait, and shews no marks of the curvature Mr. Reid has thought proper to bestow upon it, previous to his comments upon its shape.

We think it needless to expatiate more upon this subject. The copy of Thiout's book, which is now before us, is in direct opposition to the account Mr. Reid has given as an extract from it; and we cannot but suppose that the whole edition, which has been in circulation for more that sixty years, contains the same text and figures without variations. If Mr. Reid, however, can shew a copy with the description and figure as given by him, we shall willingly acquit him of the charge of misrepresentation; but, even in that case, we must still insist upon the fairness of our observations, which are warranted by the nature of the case, and called for by the interest we take in the history of Chronometry.

If there be any edition of Thiout to justify Mr Reid's drawing and description, he must be acquitted of misrepresentation. But the arguments here offered will still hold.

Mr. Reid seems animated by a violent desire of finding new things in, and deriving extraordinary conclusions from, publications which are generally known. He asserts that the invention of the compensation balance is due to Mr. Harrison; and quotes as a proof the following passage of a letter from Mr. Mudge to Count de Bruhl; "You will now permit me to speak a word or two, as to the compensation for heat and cold in the balance. It is the original method by which Mr. Harrison attempted to correct the error, which, as he was pretty tenacious of his own opinion, he carried into execution contrary to the advice of Mr. Graham, but found by experience that Mr. Graham was right, and was forced to throw it all away, and to contrive his method of applying it to the balance springs *."

Mr. Reid asserts that the compensation balance is Harrison's.

We have transcribed the whole of the extract given by Mr. Reid; and we now ask, What does Mr. Mudge's statement prove, even supposing his information perfectly accurate?

But Harrison did not complete it, and certainly never published it.

* This passage is in p. 150 of the correspondence published in 1799 by the son of Mr. Mudge.

accurate? viz. that Mr. Harrison attempted to provide the compensation for heat and cold in the balance, and that he miscarried so decidedly, that he was obliged to apply it to the spiral spring. How he endeavoured to accomplish it, is not known; therefore, any other person might be afterwards the real inventor of the same contrivance, and have the additional merit of succeeding in an undertaking where so great a genius had failed. Upon this ground, although the letter above quoted has been now published some years, and the report contained in it had been circulated long before its date, the invention of the compensation balance has been, in this country, generally ascribed to the late Mr. Arnold; and certain it is, that if there existed no other reason to invalidate his claim, the memory of that artist would continue to be accompanied with the credit of that important invention. But the invention of the first compensation balance that ever was executed, (that of the fluid thermometer), as well as that of the compensation balance upon the principle now universally used, are clearly due to P. le Roy; and the merits of the late Mr. Arnold, as an original author, in this respect, merely rest upon the supposition that he possibly may have had no previous knowledge of P. le Roy's writings. This, however, is foreign to our present object; and we shall conclude by remarking, that it is very probable Mr. Harrison never thought of using his metallic thermometer in the construction of the balance; this method being so simple and certain, that, if he had hit upon it, it could not have failed in his hands. From a passage in his last work*, it also clearly appears, that what he had in vain sought for was, the construction of a balance similar to his gridiron pendulum; and, as this passage has been frequently quoted in a mutilated manner, to shew the great difficulty and importance of the present method of compensation, we shall transcribe it entire, in order to produce the grounds upon which our opinion is founded: "And I can now boldly say, that if the provision for heat and cold could properly be in the

It is generally ascribed by the English to Arnold;

But it belongs to Le Roy;—

— and subsequently to Arnold, if we suppose him to have been ignorant of that artist's works.

If Harrison had thought of it, he would have completed it;

— but his researches were directed to the gridiron.

* A Description concerning such Mechanism as will afford a nice, or true Mensuration of Time, &c. 1765. p. 103.

balance

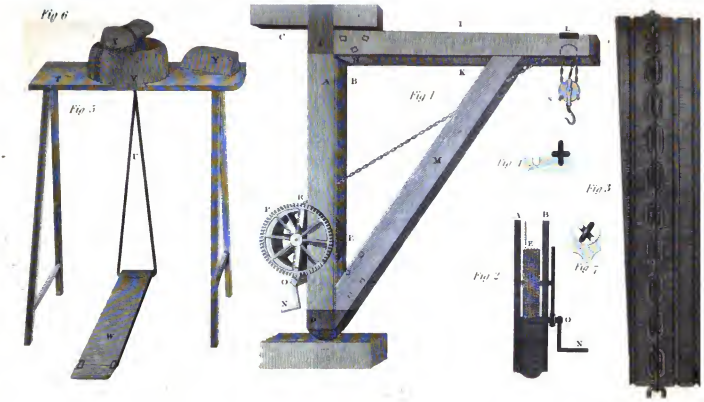


Fig 6
Fig 5
Fig 1
Fig 4
Fig 3
Fig 2
Fig 7

balance itself, *as it is in my pendulum*, the watch (or my longitude time-keeper) would then perform to a few seconds in a year."

Indeed, after the compensation for heat and cold was perfected in the pendulum, the first idea that would naturally occur to any person who wanted to correct the same errors in watches, would be, the application of a similar contrivance to the balance; and the method of a thermometer acting upon the spiral spring, could only be thought of in consequence of the first attempt proving abortive. But, let us repeat it again, in whatever manner Mr. Harrison tried to effect a compensation in the balance, the fact is, that he did not succeed; and, as his ideas upon the subject were never communicated to the public, the example of his endeavours, far from lessening, rather increases the merit of those artists who have actually accomplished that great desideratum in Chronometry.

His first attempt was probably in this way, and,

— was abandoned for the compensation curb.

R. M.

XVI.

Description of a Machine for the use of Shoemakers. By Mr. Thomas Parker*.

Mr. Thomas Parker, the inventor of the machine, was desired to attend with it upon a committee appointed by the Society of Arts, on the 22d of November, 1804, and then informed them, that he had used this apparatus for twelve months past, and found it very useful. That all the work of shoe-making may be done with it standing; but that in some parts thereof he finds an advantage in using along with it a high stool; and that before he used this machine, he never saw or heard of a similar invention; and that he has found it of great service to his health.

Machine for shoemakers.

He stated the cost of such a machine to be about two guineas.

* Communicated to the Society of Arts, who gave a premium of fifteen guineas. See also our Journal, XIV. p. 155.

Fig.

Fig. 5. Plate III. Fig. 5, 6.—T. A bench standing on four legs, about four feet from the ground.

V. A circular cushion affixed to the bench, in the centre of which cushion is an open space quite through the bench, through which hole a leather strap U is brought up from below. This strap holds the work and last firm upon the cushion in any position required, by means of the workman's foot placed upon the treadle W.

X, Shows the last upon the cushion, with the strap holding it firm.

Y, An implement used in closing boots.

Z, A small flat leather cushion, useful in adjusting the last and strap.

Fig. 6. The shoe-last shown separate from the cushion. The round cushion is formed of a circular piece of wood, covered with leather or stuffed with wool or hair to give it some elasticity.

XVII.

On the Propagation of Electricity. By Dr. OERSTED*.

THE internal mechanism of the propagation of electricity, has not, I believe been hitherto explained. It is certainly very difficult to trace all the mystery of this process, but it is certain that many interesting consequences may be deduced from the very nature of the subject, and from the facts already known.

Phenomena of communication of electricity.

The first action of an electrified body upon one which is not electrified, is, as every body knows, to establish an electric polarity. Let *A*. Fig. 2 Plate III. describe an electric body; *B*, *C*, a cylindric conductor; *B* will acquire a negative electricity, whilst *C* becomes positive. This is denominated the *communication of electricity*. It is known also, that if the extremity *C*, of the conductor, be deprived of its electricity, the conductor will retain only the power of *B*, but on bringing *A* and *B* into contact, the contrary

* Inserted in the Journal de Physique, Vol. LXII. May 1806.

contrary electricity is established in both A and C, which act is called the *electric distribution*. Communication is evidently the first, and distribution or division the second indication of electricity. We may denote the first act by the term, *first degree of electricity*, the second by *the second degree of electricity*. The former is a polarization; the latter an identification. By these denominations, we may avoid, even in expression, the false notion of a distribution. The electricity of the body A cannot be communicated from O to C, without employing some time, however short it may be. To explain this more clearly, we will imagine, after the manner of mathematicians, that space and time are divided into an infinitude of minute portions. Let us suppose the space infinitely small, wherein an electric polarity is excited; if, for example, during the first infinitesimal time, B be positive, $B\,b$ will be negative in B, and positive in b: during the second moment it will endeavour to augment the negative zone: consequently the positive zone will in like manner be extended, whilst the positive zone of B endeavours to establish a negative one farther off towards C. This process will continue till the negative electricity extends over the whole of the nearer surface of the cylinder, and the positive over the remoter surface, whilst the middle remains indifferent.

Zones of plus and minus.

—inferred to extend through all conductors.

The above process is to be understood as *continued* and uninterrupted, though for the sake of elucidation, and in order more clearly to describe the internal action of electricity in its propagation, it is here represented as *discrete*.

The propagation of electricity depends on the laws already laid down; for, admitting that each electricity excites its opposite, it is the very nature of the thing that it should be so propagated. But philosophers require to see their theories confirmed by nature in all points and under all circumstances. It is our intention to shew these proofs.

The electric fluid passes through good conductors at the rate of about a German mile in a second. In so rapid a course, it is impossible to follow the successive changes of negative and positive electricity: but with bad conductors

Electricity passes thro' one German mile per second.

ductors this may in some measure be effected. Hold a rod of glass, resin, or sealing wax, towards an electrified body; and on observing it with an electrometer, you will perceive the alternate zones of opposite electricity. This experiment is known to all philosophers.

It is needless to remark that we do not now speak of those infinitely minute changes from positive to negative, above spoken of, which, as we have already observed, we can never hope to distinguish; but to give a general notion of them, from their operation in these bodies. It might, however, be possible, to describe mathematically the number and properties of these zones.

The zones may be seen on bad conductors.

It is admitted that the foregoing mode of the propagation of electricity may be traced on bad conductors, and that it may even be observed in the air. We have then a right to consider the propagation of electricity as proceeding from *undulation*; which may be proved by other experiments. We cannot follow with the electrometer the rapid propagation of electricity on good conductors; but it frequently leaves traces upon them which confirm the opinion just advanced.

—and their effects in exploded wire.

If we attempt to melt a long thread of iron by means of a weak charge from the electric battery, we shall quickly perceive that one part of the thread is fused, whilst another remains entire, and that these parts are alternate. If a stronger charge be employed, the whole thread will be fused, and formed into small globules, which are produced by the expansive and contracted zones. The charge may be so managed as to give a red heat to the metal without fusing it; on which will afterwards appear evident marks of the transition of expansive and contracted zones.

The foregoing experiments are all well known to phisophers, and afford the strongest proofs of the undulatory propagation of electricity. But if the charge of the battery be augmented to such a degree as to volatilize the iron, and the experiment be so contrived as that the vapour may be caught upon a sheet of paper, we shall have a complete image of the propagation of electricity, in the clouds described upon the paper by the alternate transitions from expansion to contraction. The thickness

even

even of the smoke, and its colour change so regularly, that we may call it a coloured portrait of the oscillatory expansion of electricity. From the constant recurrence of the phenomenon, it is evident that this appearance is not an accidental effect, for let the experiment be made with any metal, the result will be similar without exception. Van Marum has in numerous and faithful representations explained this experiment, which may save us the trouble of repetition. The regularity of this image may be seen by another method. If electricity acted on the metallic thread by an expansive force solely, all the clouds of the vapour would be parralel and straight; but as each conductor acts with a repulsive force on the nearest extremity of the thread, the clouds of vapour thrown to the two extremities by two powers which cross each other perpendicularly, follow the diagonal of those powers or rather, as the powers are constant and unequal, the clouds represent the image of a curved line, whose concavity is opposed to the metallic thread. The more distant a cloud is from one of the conductors, the less will it be affected by the repulsive force parallel to the thread, and the nearer will its position approach to the perpendicular force of the wire.

The smoke of metals exploded by large batteries.

Van Marum's experiments.

In the middle of the wire exists a perfect equilibrium of the opposing powers; and consequently the position of the shade will be exactly perpendicular to the wire.

The foregoing appearances will not be obtained if the force employed to evaporate it be too strong; but even in this case the figure will describe a zigzag, at each section of which, indications of the above order may be discerned.

An examination of the electric spark will afford us another proof. If the conductors, between which the spark appears, be pretty close, the spark is differently coloured at its two extremities, being red on one side and blue on the other, whilst the centre is white; but if the conductors be placed further apart, the spark will vary its colour as often as it makes transitions from positive to negative.

The electric spark shows undulation.

All that has been here observed of electricity is equally applicable to magnetism. The action of the load-stone originates

Undulation or zones observable in magnetism.

originates in polarization, and like electricity, communicates its powers with an undulatory motion. One zone of polarity must acquire its maximum of expansion, and thus give rise to another. This is confirmed by experience; for in magnetising a very fine steel wire, it acquires the alternate poles of north and south, in its whole length.

To understand the propagation of magnetism, we must reflect a little upon its production; and examine what effect is produced by drawing a magnet over a bar of steel: the two poles are impelled forward, so that the part which was $+m$ becomes $-m$; like a wave of the sea, which fills up a furrow before it, whilst it leaves another behind.

All the operations of nature are thus propagated.

This mechanism in the action of undulatory propagation, is doubtless general in all the operations of nature; but it is very difficult to shew it. It has long ago been observed, that the compression of a small portion of air is succeeded by its expansion, whereby contiguous portions must be compressed, and these, by expanding in their turn, compress others, &c. It is thus that the communication of sound through the air has been accounted for; but this mode of communication has not yet been suspected to take place through solid bodies.

Chladni's experiments.

The majority of philosophers oppose the discovery of the celebrated Chladni*, of the tremulous motion of the particles of bodies in the production of sound. But nothing is more easy to demonstrate both from the nature of the thing, and by experiments the necessity and the existence of this tremor. We need not much insist upon the theory, because the same proofs which are adduced to demonstrate the undulatory communication of sound through the air, may be applied to all other bodies; besides as motion cannot be communicated without employing some portion of time, all the particles cannot be similarly affected at the same moment.

Experiment with a wire and Lycopodium.

This is manifested in the following experiment: cover one of the extremities of a steel wire with powder of Lycopodium, and then strike it with a sharp and moderately

* Inserted in the Journal de Physique, Vol. 47, p. 390.

hard

hard blow, the powder will be divided into small heaps, describing a line, the length of the wire; those nearest the point struck will be the largest, the others will gradually diminish in size as they lie further off. This experiment may be made in a still more simple manner: take a square of glass or metal, whose edges are quite even, cover the surface with powder of lycopodium, and hold it with the fingers by opposite sides, leaving the other sides quite free. If one of the free sides be struck with a piece of wood, the powder will be immediately thrown into lines parallel to the direction of the blow, in which may be observed many elevations and depressions. But if the blow be given with a rough board, on the whole extent of the edge, the powder will dispose itself in lines parallel to the side struck. The lines will be more or less wavy, in precisely the same degree as the side struck was more or less even. If one of the surfaces be struck, a number of little heaps will be formed. This is doubtless the result of an oscillatory movement, and most decidedly of a progressive and an undulatory motion.

—and also with a square plate.

But if the tablet be held between the finger and thumb upon the two surfaces, without touching the edges, and a blow be given on the upper surfaces, not only heaps are formed by the powder, but a sound is emitted. The heaps receive a motion which obliges them to reunite at the extremity and they nearly assume the figure described by Chladni. Prepare the whole as if to obtain the figure of Chladni, using lycopodium instead of sand, and the figure will gradually appear. At the first blow the heaps will be formed like small knots, ranged about the points where the largest were formed. Let $ABCD$ (Fig. 3, Pl. III.) represent the tablet struck on the point E; the heaps described by $e\acute{e}$, ff', gg', &c. will be formed: $e\acute{e}$ will be formed sooner than ff', and ff' sooner than gg'. The first on the point E, and on all the line EE', will be driven towards E'; but that on e and $\acute{e}$ will be determined by two powers in the direction EE or ED and EE', it will then describe the curved line $\acute{e}h'$, and all the other points will traverse similar lines.

Figures formed by lycopodium on glass, &c.

In this manner the curve $CE'D$ will be described; and as all the other squares of the tables $AE'C$, AEB,

A E D, will have received oscillations at the same moment, they will form as many curves, altogether forming a kind of cross or star. It is to be observed that the lines at rest are not described by heaps of dust, but are surrounded by them. These lines cannot be described in the ordinary manner by the scattered sand, because it is elastic, and its particles are too considerable for each one to continue in motion untill it finds a tranquil and appropriate place. If two grains of sand be thrown upon a square of glass, thrown into motion by means of a bow, the experiment will confirm what has been just laid down. Consequently, the lines of powder must not be confounded with the lines of repose, which are denominated lines of knots. *The powder, on the least motion,* detaches itself from the lines of knots, as is seen in the experiment; but it is very difficult to detach it from the lines of sand. Hence we may suspect that the undulatory movement excites a degree of electricity, which is doubtless negative in the lines of knots and positive in those of repose, since the negative lycopodium is attracted by it. It is probable that all philosophers are acquainted with what Ritter has said on this subject in Voigt's Magazine.

LECTURES ON SURGERY.

Mr. A. CARLISLE, F.R.S. F.L.S. and Surgeon to the Westminster Hospital, intends to deliver a Course of Lectures on the Art and Practice of Surgery, in all its Branches, during the present Season, at his House in Soho Square.

The History and Treatment of the Diseases and Affections, which belong to the Province of Surgery, will be fully investigated; the several Methods of Practice examined, and accompanied with such Observations as the Lecturer's Experience may furnish. The various Chirurgical Operations will be demonstrated, and the Anatomy of the Parts explained, together with the Deviations, Accidents, and Difficulties which occasionally happen, and the Rules to be observed in each Instance.

An Introductory Discourse, (open to all Students) comprising the Plan of these Lectures, will be given on Monday, October 13, at Seven o'Clock in the Evening. And the Lectures will be continued on Mondays, Wednesdays, and Fridays, at the same Hour.—Tickets for the Course, Three Guineas; perpetual, Five Guineas.

A

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AND

THE ARTS.

NOVEMBER, 1806.

ARTICLE I.

Experiments and Observations on the Adhesion of the Particles of Water to each other. By BENJAMIN, *Count of Rumford, F. R. S. &c. Communicated by the Author to the National Institution of France, and transmitted to him by the Editor.*

(Concluded from page 159.)

TO prove this fact in another manner, I again varied the experiment, by placing a stratum of ether immediately upon the mercury. The particles of this liquid appear to have very little adhesion to each other; for which reason I imagined that the kind of film that would be formed at its surface, must have very little force. The results of my experiment fully confirmed this conjecture. A stratum of ether upon mercury—

The very smallest spherules of mercury which I let fall through this liquid, seldom failed to mix immediately with the mass of mercury on arriving at its surface, where they entirely disappeared; and I have never succeeded in causing either a spherule of mercury, or the smallest metallic particle, nor any other body of greater specific gravity than ether, to swim upon its surface. — appears to afford no resistance to descending bodies.

Alcohol.

The results of the experiment were not perceptibly different when alcohol was substituted in the place of ether.

The evaporation of ether—

It is known that ether evaporates very rapidly. Is not this another proof that the particles of this liquid adhere to each other with much less force than those of water? But the following experiment proves this fact in a decisive manner.

EXPERIMENT VII.

is incomparably greater than of water, and shews less adhesion.

Having half filled a small cylindrical glass with mercury, I placed on the mercury a stratum of ether four lines in thickness, and blew upon the ether with a pair of common bellows.

In less than one minute the ether had disappeared.

The same experiment being made with water, no sensible quantity of this fluid had disappeared in one minute.

Dust, which has no adhesion, rises by the wind;

The objects which are before our eyes from the earliest periods of our lives seldom employ our meditation, and not often our attention. We see, without surprise, immense masses of dust raised by the winds and carried to great distances; and at the same time we know that every particle of this powder is really a stone, almost three times as heavy as water, and of a size so considerable, that its form may be perfectly seen by means of a good microscope.

And we see also, without surprise, that water, which is much lighter than dust, and is composed of particles incomparably smaller, is not carried off by the winds in the same manner.

—but those of water do not.

In order to convince ourselves that the particles of water do strongly adhere to each other, and that they require to be so in order to prevent the greatest confusion in the universe, we need only figure to ourselves the inevitable consequences that would result from the want of such an adhesion.

If they did not adhere, they would rise more easily than dust.

The particles of water would be raised and carried off by the winds with infinitely more facility than the finest and lightest dust. Every strong breeze setting in from the Ocean would bring with it a great inundation. Navigation would be impossible, and the banks of all the seas, lakes, and large rivers would be uninhabitable.

The

The adhesion of the particles of water to each other is the cause of the preservation of that liquid in masses. It covers the surface with a very strong pellicle, which defends and prevents it from being dispersed by the winds. Without this adhesion, water would be more volatile than ether, and more fugitive than dust.

But the adhesion is also the cause of other phenomena, which are of the greatest importance in the phenomena of nature.

The viscidity which results from the mutual adhesion of the particles of water renders this fluid proper to hold all kinds of bodies in solution; as well the most heavy as the lightest; provided always that they be reduced to very minute particles. Hence all bodies may be suspended in water.

I have found by a calculation, founded on facts which appear to me to be decisive, that a solid spherule of pure gold, of the diameter of one 300,000th of an inch, would be suspended in water by the effect of its viscidity; even though this small body should be completely wetted and submerged in a tranquil mass of the fluid *. Calculation of their size.

This viscidity, or want of perfect fluidity, which causes it to hold every kind of substance in solution, renders it eminently proper to become the vehicle of nourishment to plants and animals; and we accordingly see, that it is exclusively employed in this office. The nutriment of plants and animals is thus conveyed.

If the adhesion of the particles of water to each other were to cease, and the fluidity of this body were to become perfect, every living being would perish by inanition.

May I be permitted to remark the simplicity of the means employed by Nature in all her operations—May I be permitted to express my profound admiration and adoration of the Author of so many wonders!

* Fragments of gold leaf, which are about one-280,000th of an inch thick, subside in water with considerable velocity. This, however, does not invalidate the argument in the text. N.

II.

Abridged Extracts relating to the means used to reduce the weight of Horse Jockies and the methods of Training Horses, so as to augment their Strength, Wind, and Speed. From Sir JOHN SINCLAIR'S *Pamphlet on Athletic Exercises.*

SIR Charles Bunbury transmitted a letter from W. S. Rickword, Esq. who after mentioning some of the difficulties of collecting information from many of the persons of the class who practise the arts in question, proceeds to give the following observations, p. 65.

Physic is not much used in training men or horses.

"Physic of no kind is used now, so common as it used to be, either in wasting men to ride, or in training them to pugilistic engagements, or extraordinary muscular exertions of any kind. The number of questions put by this author might be compressed into a very few; like summoning and capitulating commanders of armed men, many of the articles are said to be answered in the foregoing, number so and so, as the numerous questions* (made use of in this pamphlet) are (in a great degree) by the following general observations. The good effects of air, exercise, and aliment, to animal bodies, to the attainment of health, is tolerably well known. No general rule can be laid down as to the mode of feeding; the quantity of exercise, or the time required, to bring either man or horse to perform the utmost he is capable of doing: the conformation, and idiosyncrasy of the body of each animal, the trainer should make himself acquainted with; men and horses differ in constitutions, as in dispositions. The great art amongst trainers is, *or should be*, to discover what quantity of exercise, &c. a horse will take to bring him to, and keep him at his best. As to physic, it is my opinion, that it is much too generally in use amongst racing horses in particular; but, upon that subject, I have more to say than is convenient for me to advance at present. More depends, far more, on exercise than is generally believed, even at this period, though the benefit is pretty well known and admitted; yet, by no means

Feeding not subject to any general rule.

Exercise, air and thorough grooming required by horses.

* See our Journal.

sufficiently;

sufficiently; pure air, proper exercise, good oats and hay, with thorough grooming, would bring horses to the starting post, far better able and in condition for running than they usually are brought, in consequence of the *too common* use of physic, and the quantity given at each dose. I am persuaded that alterative medicine would answer a better purpose than stronger physic, in most cases, where, even it is exhibited judiciously; I do not say, that physic is at no time proper, there are situations, when it is highly necessary; but I contend against the frequency of its exhibition, and the quantity exhibited; I do so, thoroughly convinced of its laying the foundation of some diseases, and rendering the animal incapable of contending against any other, with which he might unfortunately be attacked. As to the food used in the training of men, I should consider that which affords the most nutriment, occupying the least space, and digesting easy, to be the most proper, and likely to give the greatest assistance to the other requisites, in training them to perform any feats requiring extraordinary exertion of the muscular system; this attended to, with the benefit of free respiration (without which, nothing great can be performed, either by man, horse, or other animal) will admit of astonishing and wonderful powers and strength, either in wrestling, pugilism, walking, running, &c. &c.

Food for training men.

"As far as relates to strength and wind, the foregoing observations apply to fowl, as well as other animals. Fighting of all kinds I am an enemy to; cocking I never see, nor do I like to hear of it. The foregoing observations are hastily written, but rest on the best foundation."

Fowls.

Mr. Sandevir, an eminent surgeon at Newmarket, returned in substance, the following information to Sir John Sinclair.

The training of jockies of high repute is continued or kept up, more or less, from about three weeks before Easter to the end of October, which is about eight months: but a week or ten days are quite sufficient for a rider to reduce himself from his natural weight to a stone and a half below it. They breakfast very sparingly on bread and butter, with tea; dinner, fish, or else pudding with very

Particular account of the training by which jockies are reduced in weight, &c. It consists in taking little food and sharp exercise.

very little meat; wine diluted with twice its measure of water is their drink: tea in the afternoon with little or no bread and butter, and no supper. It appears that abstinence is their principal object.

As to their exercise, they load themselves with clothes, that is, five or six waistcoats, two coats, and as many pair of breeches; in which dress they take a severe walk of fifteen or sixteen miles after breakfast. On their return, they change their clothes for dry, and some who are much fatigued, will lay down for an hour before dinner. No severe exercise is taken after dinner; but the day is passed as they please. They generally go to bed at eight or nine, and rise about six or seven.

Those who are unwilling to take excessive exercise, have recourse to purgative medicine; which usually consists of two ounces of Glauber's salts.

This treatment recommended against corpulency.

Mr. Sandevir is positive in recommending a similar process for reducing corpulency in either sex, as from experience he perceives that the constitution does not appear to be injured by it: but he is apprehensive that very few indeed could be prevailed upon to submit to such severe discipline, unless he had been early inured to it.

Extreme case of sudden reduction.

This gentleman mentions as an additional fact, that John Arnall, when rider to the Prince of Wales, being desired to reduce himself as much as he possibly could, for a particular purpose, abstained from animal and even from farinaceous food for eight succeeding days, and eat only a piece of apple now and then. He was not injured by it at the time, and is now in good health. The writer also adds that Dennis Fitzpatric, a person at this time continually employed as a rider, declares that he is less fatigued by riding, and has more strength to contend with a determined horse in a severe race, when moderately reduced, than when allowed to live as he pleased, though he never weighs more than nine stone, and frequently has reduced himself to seven stone seven pounds.

Another person answered the queries to the following effect.

Another account of jockies, &c.

Jockies are trained and reduced by abstinence and by sweating, in consequence of additional cloathing and long continued

continued walking. Neither their health nor their strength are impaired unless these practices are carried to excess. When much reduced they are peevish and irritable, but perhaps not less courageous than usual. Many of them are naturally lean, but some recover their weight very rapidly when the course of training is left off. Neither their health, nor their continuance of life appear to be affected by this practice.

Mr. Robson, an eminent trainer at Newmarket, gave in substance the following information respecting race horses.

Method of training race horses for the course.

The running horse is of a superior race.

Exercise, cleanliness and good provender constitute the treatment.

The perfection of a race horse consists in his wind, which is innate in their breed, and degenerates when mixed or crossed with other horses. It is observed sometimes that the other species of horses go nearly or quite as fast as the slower kind of race horse, but they very soon tire for want of wind, whilst the running horse has the peculiar merit, from his wind, of bearing fatigue so much better than any other breed of horses. The perfection depends on their parentage and on the female most. The foal must have corn during its rearing, otherwise it will not grow in proportion, but grow lean in the haunches. Different individuals of the same family will greatly differ in their natural constitution. Good size, with strength and symmetry of form, are essential to the running horse; but the most essential qualities are activity in speed, and good wind. With regard to form, he should be broad, deep, and have great declivity in his shoulders, his thighs let down very low, the hocks stand far behind and from him, thence downwards to the next joint, short, &c. large bones are preferred. Each sex is alike for speed, but the horse bears fatigue better. The foal is kept in grass fields in the state of nature till broke, and well fed with corn, as he will eat it, and with hay where grass is scarce. The training is began at two years and a half. Soft meal is a cooling food, but laxative and injurious when horses are at hurrying work.

Physic, food, &c. for race horses.

Race horses are purged two or three times a year; each course, perhaps three doses preparatory to their getting their training exercise. Mild physic which has no tendency to weaken, is made use of. (I suppose this to mean a moderate

a moderate dose). Oats are the most esteemed provender for horses; and of these they have three feeds daily, of as much as they can eat with appetite. Their drink is soft water at least twice a day, always cold, except during physic or illness. Their skins are kept perfectly clean when in the stable, by friction with the brush and curry-comb, which clean and brace the skin and muscles. It is necessary to health and strength that they should be sweated, and this is done by putting on a few extra clothes and cantering them five or six miles according to their age and other circumstances. They are exercised twice a day; a mile or so in a gallop before they take water; and afterwards a short or long canter, as circumstances and their constitution require. The training is completed by good keep and a proper proportion of work, which enables them to bear fatigue. This is kept up for two or three months only, and effects no more than a temporary change in the animal. Running horses certainly live as long as others; they are not sooner worn out by the treatment they undergo, but on the contrary they bear fatigue much better than other horses.

Mr. Holcroft's account of running horses and their treatment

Mr. Holcroft's observations in the same treatise, nearly coincide with those of Mr. Robson. This celebrated dramatic writer lived at Newmarket, in his youth, under John Watson, the groom, who was employed in the two-fold office of training the horses and riding them. John Watson died at a very advanced age. I quote Mr. Holcroft's words, page 77.

They are purged and exercised.

"When the racing season is over, these horses have most of them green meat for some time, and repose from their severe exercise; their high spirit and vices soon begin to shew themselves, much to the terror of timid boys. Having fed grossly for a time, they are regularly purged, I forget how often, but I believe every other day, for three doses; and that these purgations are repeated, at intervals, three times. They then gradually begin to increase their exercise, so that, early in the spring, they remain out of the stable about eight hours in four-and-twenty, and take what are called four brushing gallops, two in the morning's exercise and two in the afternoon's; a brushing gallop means a gallop of nearly a mile, beginning

ning at a moderate rate, increasing, and ending full speed. They are stinted in their water; the horse that blows the hardest, the most; their hay and oats are of the best quality; the hay is long in the stalk, and the seed shaken out; the oats are thrashed in a sack, and winnowed, and every care is taken to keep the horses from chaff and impurity of every kind. After feeding, their heads are muzzled. They are not allowed above six hours in the night; for they are supped up at nine, and out again at three in the morning; but they have the intervening hours in the day, between their morning and evening exercise. When they become wet, from the accidents of weather, or other things, they are carefully rubbed till dry. Each horse has a boy for the performance of all these particulars; they are occasionally sweated, I forgot how often; that is, they are heavily clothed, galloped nearly full speed for four miles, relieved from their violent perspiration, first by wooden scrapers, then by rubbing them till they are perfectly dry, and after a little gentle exercise, are taken home.

Stinted in their water.

Method of sweating.

I have spoken to the best of my memory of things that happened at least six-and-forty years ago, and concerning which, when I quitted Newmarket, I never imagined I should be more questioned. The skins of the horses are kept perfectly and peculiarly clean; severe perspiration is thought absolutely necessary. I see no reason to suppose that their lives are shortened; some of them live to a great age. Eclipse, I think, died above thirty.

They are not short lived.

III.

Second Letter from R. B. *on the Developement of Intellect and Moral Conduct in an Infant, during the earliest part of her Existence; being concluded at the fourth Month of her Age.*

To Mr. Nicholson.

Sir,

I thank you for inserting my paper on the Progress of Intellect in an Infant, and have now the pleasure of send-

ing you the remainder of my notes on the same individual. As I have made it a particular point to keep close to what was actually written at the time, instead of trusting to any thing my memory might now suggest, you will find some repetitions and perhaps defects of style, which that resolution has prevented me from amending. The dates continue to express the age of the child, whose progress from one day to another became less marked, in proportion as her stores of knowledge and acquirement became greater when compared with the improvement of any short interval of time.

Continuation of a register of the progress of an infant from twelve days old.

15th day of her age. Knows her mother at a short distance

Fifteenth day of her age. The infant decidedly knows her mother when near her; but doubtfully if distant. She has long known her when in her arms. Her acuteness of observation and the use of her hands improve, though slowly. She grows very fat and is indolent, probably from the constitutional habits of her age and growth, and perhaps from the less lively impression of surrounding objects to which she is now accustomed. I think she still shows me a marked preference of intelligent attention. This morning her mother was talking to her, and upon her giving some striking signs of pleasure, her mother called to me "do look at her,"—the infant instantly turned her head from her mother to me, and appeared highly pleased at my coming to her. This could scarcely be casual: if it was not, she must have made considerable progress in the knowledge of the shortest and most frequent sentences used respecting her, of which "do look at her," is certainly one of the most frequent.

—and is aware of the use of language.

19th. Diverted by other children.

Nineteenth day. She is highly interested and diverted at her brothers and sisters, who are running about the room and occasionally take notice of her.

23d. Endeavours to articulate.

Twenty-third day. The infant is very desirous of articulating, and makes many efforts, by varying the form of the mouth and position of the tongue. When she succeeds in producing the resemblance of a word or syllable she is much pleased and shews her satisfaction by motions of the legs and arms. She never makes this effort but when engaged and attentive to some person who speaks to her, and whose approbation she seems to court by an endeavour at imitation.

26th day. More perfect in articulating:

Twenty-sixth day. She can without any difficulty utter many voluntary simple sounds at pleasure. Her wants being now more numerous and habitual, she betrays more impatience than formerly, at privations or inconveniences. This impatience appears to be grounded upon a moral deduction that she has a claim or right to be indulged.

It is to be observed that her health, appetite and sleep have hitherto been perfect, and she has become much fatter than at the earlier part of her life.

—knows that the word child denotes herself

She was baptized three days ago, and of course does not yet know her own name; but she has long known the word "child," as denoting herself.

6th week. Knows her own name and various other words.

Sixth week clapsed. The infant very decidedly knows that the word "mama" denotes her mother, which word pleases her more than any other, except the word "child." She knows her own name and attends when called by it. Conversation fixes her notice, even though not addressed to herself; and she can utter many sounds without hesitation or effort, in the imitation of conversing or answering, very differently from her manner a few days ago.

7th week.

Seventh week. Her preference, directed to her mother, myself, and other favourites, are now expressed in a variety of ways. She holds out her arms and leans beyond the equilibrium, in order to prevail on us to take her from her maid, at the same time that her looks and voice are perfectly intelligible and expressive.

9th week. Attends to general conversation.

Ninth week elapsed. C—— is now more pleased to listen to distinct general conversation, than to common phrases addressed to herself. I suppose this preference to arise chiefly from the greater variety of tones and articulate sounds which are new to her, and perhaps from the interest she may take in the concomitant action of the speakers. Her general habits and use of the eyes have gradually improved in accuracy and minuteness. She has nearly, but not perfectly, a command of the vertical position of the head; and turns to the place whence a voice addressed to herself, proceeds. But she does not always perform this last action with certainty and precision. In the use of her hands she gradually improves; and particularly exerts this action in feeling or pinching the breast

—can support the head and look round by acting with the muscles on the neck.

 while

Uses the hands but not in connection with the eyes.

while sucking. There is yet, however, very little connection between the hand and the eye. While she is at the breast, contemplating her mother's face, she occasionally stretches forth her arms and is delighted if her mother will lean forward and kiss her hand. Her smile, which originally seemed to denote simple pleasure, is now more expressive and intelligent. She laughs at being mocked or suddenly deprived of the breast; and waits with some eagerness for a repetition of the trick. The nurse's practice of covering the child's face with her pin-cloth, and then suddenly plucking it off affords her diversion. Keys rattled before her, or a nosegay held near her, are viewed with eager attention and prominence of the mouth; at the same time that she grasps her own clothes, but does not attempt to apply the hand to the object of attention. She has a very marked fondness for her mother, and shews it, occasionally, by applying her mouth, opened very wide, against her mother's cheek, making at the same time, a gentle noise expressive of affection.

Pleased with tricks and mockery.

Very attentive to objects held to her; but does not try to take them.

10th week. No attempt to seize visible objects.

Tenth week. Though her improvement in connecting the action of the hands with the sense of sight is very evident in her manner of taking hold of her own clothes or her mother's neck-handkerchief, yet she makes no attempt to seize any thing, in consequence of first seeing it.

Peculiarity in the infant face; noticed by Hogarth:

Hogarth in his "Analysis" of beauty mentions as one of the characters of the infant face, that the iris or coloured circle of the eye, being nearly of the same size in all ages, bears a greater proportion than usual to the size of the face in young subjects. But there is another more striking and very general difference. In this infant, the bony edge which supports the eye-brow, being naturally low, the upper eye-lid at first covered part of the iris as it does in many adults; but when the face became full and prominent, as is the case with thriving children, the lower eyelid, being pressed upwards, covered more of the iris, than the upper. This effect is common with infants, but is, I think, never seen at a more advanced age.

—another still more remarkable.

11th week. Attempts to speak or answer by sounds

Latter end of the eleventh week. The attachment of C—— to her mother seems to increase. She laments or whines when the servant carries her away. Her attempts to

to speak improve in manner and precision of answering when spoken to, which she does by a sound sometimes of pleasure and sometimes of mere assent or attention. These sounds considerably resemble those of a monkey we had some years ago, which was habituated to reply to kind language. Her mother, as well as myself, thinks C——'s power of mind and observation are at this time much superior to that of the monkey; but her education, or quantity of acquired habits, less. —resembling the language of a monkey.

C—— refused to go from her mother to her eldest sister, but readily left her to come to me. She knows when her maid, though absent, is called to take her. The amusement of spinning a half crown on the table diverts her much, but she makes no attempt to seize it. If however it happens to touch her hand, she is greatly entertained, and seems to have a notion of possessing it. Spinning a piece of money.

Twelfth week, or age nearly three months. The variety of tones and what may be called words which C—— can now command, are sufficient to make herself perfectly understood, as to pleasure or pain or the mental affections, without crying; and she certainly understands quite enough of language to apprehend all that her wants and powers require to be communicated. She does not yet attempt to seize any object, with her hand under direction of the eye. 12th week. Makes herself well understood.

End of thirteenth week. C—— having been ill with a complaint in the bowels, has shewn the most marked partiality for me; so as to quit the breast to come to me, when I appear. I think this arose from an habitual conviction that I, as the adviser and director of the family, could do her good*. It is probable also that my greater personal strength and ability to walk about with her and also the facility with which she and I understand each other, might afford strong motives of preference, by giving her that amusement which beguiles pain. 13th week. In illness she is attached to her father.

She has often and long ago been carried to a looking glass, which amuses her. From various facts I am con- She knows that a looking glass

* The same attachment and conviction has always been manifest in the illness of her and my other children at later periods.

vinced

shows images only. vinced she at present knows that the figures are not real persons, but represent herself and others.

Three months old. First connection of the hand with the sight. End of the fifteenth week, or age exactly three calendar months. Yesterday C——, who has been assiduously watched for that purpose, did not move her hand to take up any thing; and to day, at six in the evening, she completely acted with the hand and eye in conjunction. It seems as if this operation had been projected and previously arranged in her mind. The operation was curious and seems to have been studied or planned. She raises her arm by the shoulder-joint to a level with the object she desires to take, and then by an horizontal sweep, brings her hand before her, opening and shutting the hand till she has clasped the object, in which she does not readily succeed. Anxiety and impatience accompany this manoeuvre, and, on the whole, she is a good deal vexed with the desire to possess in this new way and the difficulty of bringing her hand to the object. I think she uses her right hand with rather more success than the other. When she had, with both hands at once, grasped the tea tongs, she could not command the voluntary power of letting go and therefore cried from the confinement of her hands.

3½ months. Articulation, language, &c. Three months and a half old. That effort at articulation which nurses call telling a long story, was very earnestly practised at this period, and some days afterwards she became very troublesome, from a wish to seize whatever was in her view. That habit of tossing the arm up and down, which infants acquire, and to which some authors ascribe the use of the right hand in preference to the left, was also exhibited at about four months old*. And soon afterwards her knowledge of words and things was so far advanced, that she knew her hands and feet by She knows her

* The argument is that infants are usually carried on the right arm, because it is stronger; and in this position, the right arm of the child being at liberty, is said to be exercised more readily and early. It does not seem however, that there is much force in this remark; for the nurse is as likely to carry them on her left arm, in order to have the free use of her own right arm; and even on the former supposition, it seems to me that the arm nearest the nurse, would probably be more fully exercised by taking hold of her, or her clothes, than the other, which for the most part can have no object within its reach. B.

name;

name; that is to say, she shewed them when asked "which is your hand,"—"which is your foot," provided her attention was not turned to other objects.

hand and her foot by name.

To this period I carried my journal. The subsequent months were not noticed; and indeed in these she became one of the family as to general intercourse, making herself understood by all, and comprehending what was said to her to the full extent of her understanding and the simplicity of her wants. I shall not extend my communication by arguments and inferences; but will only take notice that children do not speak sentences, and indeed scarcely words before they are twelve or fourteen months old, though my narrative seems to shew that they possess ability to do it much earlier. On this subject I would remark that the latter part of the first year of the life of an infant, is a time of indolence; when most of their wants are supplied by attendants who are constantly with them; and in the lower ranks of society, they are so ignorantly treated that they do not speak intelligibly for years; and again that they seldom have their teeth till after the twelvemonth. I have known a child who had teeth at six months, and spoke many words very well at that period, with a knowledge of their meaning*; but though he was highly satisfied at his own performance, he did not find motives for proceeding in his labours after language, till about the fourteenth month, when he began to run about, and found his wants and views so multiplied under this new change of circumstances, as to require a greater share of diligence than he had found needful in the arms of his protectors.

The journal ends here.

Qu. Why do not children speak earlier than at twelve months?

Because their necessity for speech does not operate till they walk, &c.

—and they have not teeth.

Instance of a child who had teeth and spoke at six months; but did not persevere, nor was he more advanced at the twelvemonth than other children.

I remain,

Sir,

Your constant reader,

R. B.

* He is now a very intelligent, unaffected boy; but has no extraordinary claims to notice, either in his own opinion or that of others. B.

IV.

On the Culture of Beans preparatory to a Wheat Crop. By JOHN CHRISTIAN CURWEN, *Esq. M.P. of Workington Hall, Cumberland*.*

SIR,

Cultivation of beans and wheat, &c.

THE offer of a premium by the Society of Arts, &c. for the culture of beans preparatory to a wheat crop, being, as I conceive, for the purpose of demonstrating the superiority of green crops over dead fallows; I shall be considered, I flatter myself, as acting consonant to the views of the Society, in offering a detailed account of my proceedings, more especially as it will appear incontestably, that, if any advantage has resulted from a trial under such very unfavourable circumstances, the most sanguine expectations may fairly be entertained of the general utility of the system.

The plot of ground on which the experiment has been made, contains forty-two acres, the soil is a stiff clay, so flat as to afford very little fall for the water. The least continuance of rain renders it unworkable, though it has been drained as far as was practicable. It was broken up in the spring of 1800, and in that and the following year was under oats, both crops very heavy; in 1802 it was set with potatoes; in June, they were run through with the potatoe harrow, and made quite flat before they could be stitched up again. The wet set in and continued so long, that the crop was in a great measure ruined, and the weeds got to such a head that it was not possible to get the ground cleaned. It was sown with wheat in November 1802, by great exertions, but it was in so very unfit a situation that the greatest part of the seed perished: above half was re-sown with oats, in April 1803, being as soon as it could be got upon. Immediately after the crop was got off (early in October 1803) the stubble was turned up: in many parts the grass was

* This communication was made to the Society of Arts, in three letters to the Secretary, which I have here given without abridgement.

so thick and strong, as to make it difficult for the plough to get through it. The winter proved so mild that it had done it little good. In many parts the harrows could not break it, and the grass was obliged to be cut and carried off by the hand. The advantage of a second ploughing would have been great, but by attempting it I might have lost the season for getting in the beans; I was restrained therefore from attempting it.—Forty acres were drilled before the end of February 1804, with a drill of the construction of Mr. Mac Dougals, six feet wide, sowing the rows at twenty-six inches apart. The weeds and roughness of the land would not admit of the drills being kept exactly straight, which occasioned additional trouble in cleaning, as also some loss in the crop. Forty-nine and a half Winchester bushels were sown.—I have been thus particular, to convey a just idea of the uncommon foulness of the ground, and the difficulty I had to contend with in consequence of it. The beans came up extremely well, notwithstanding the extreme severity of the spring. No step was taken in cleaning till the 10th of May 1804; this neglect proceeded from the multiplicity of other business, and my over-man being unacquainted with the drill husbandry, and the advantages of beginning to destroy the weeds as early as possible; from the 10th of May till the middle of July, which was as long as it was practicable to continue, the ploughs and harrows were constantly employed, and it was twice hand-weeded during the time. The cutting of the beans commenced the 20th of August: had the weather permitted, it might have been a week earlier. The method followed, which I had practised with success the year before, was to cut and spread the beans thinly, and to leave them exposed to the sun two days previous to binding. By the 26th, the whole was cut, and the field cleaned by the 29th.—I gained by these means above a month, which on wet land is of infinite advantage; I had great mortification in finding, after cutting the beans, the stitches extremely foul, notwithstanding all the pains I had taken. Any thing so dirty as this ground could seldom be met with; the season was very favourable, and I began to clean it immediately; I gave it two ploughings,

Cultivation of beans and wheat, &c.

Cultivation of beans and wheat, &c.

and in some parts three, breaking it with harrows, raking and hand-picking it. I had, by the 20th of September 1804, the satisfaction of seeing it in a better situation than any fallow in the neighbourhood, and began to plough for wheat; on the 29th it was completely drilled, rolled, and water-furrowed. My friend Mr. Green, a member of the Society, who visited the field, was so struck with the busy scene, that he requested to have the people and the horses counted. There were fifty-nine men and women, and thirty-one horses; fourteen single, and one double cart, four ploughs, four harrows, drill, roller, and water-furrow plough, a horse each. It took sixty-two and a half Winchester bushels of seed; I had sixty carts of compost per acre, composed of dung, ashes, and street-rakings, that had been collected during the summer, and laid in the most convenient situations to facilitate the work. The filling, leading and spreading of 2500 carts of compost was a work of some magnitude; the month of October proved so wet, that, had it been delayed a week later, I should not have been able to have accomplished it. The labour it cost me after the beans were cut was very little inferior to a regular fallow; notwithstanding, the result, with this increased expense, will be found to be in favour of the experiment. The tick bean, which was sown on thirty-nine acres out of the forty, produced more abundantly than the other bean, which was sent me by Messrs. North and Bridge, and, being a later bean, is not adapted to this climate. The crop was good; one stalk of the tick bean had 70 pods, and these produced 353 beans; the weight, four stone thirteen pounds the Winchester bushel; the other bean, four stone four pounds. The crop produced 2010 stooks; from a few stooks which were left out of the stacks for the purpose of affording specimens for the Society, I have reason to suppose they will yield ten quarts per stook, or 628 Winchester bushels. I estimate by the London seed, which is least productive. The selling price is five shillings per Winchester, which would make the amount £167 *9s. 4d.* The stooks had been exposed to the inspection of various persons who wished to see in what state the beans were, so that I suppose some loss in the quantity,

quantity. The following is taken from the over-man's day-book, and I believe the greatest attention was paid to have the expense correct.

Cultivation of beans and wheat, &c.

	£.	s.	d.	£.	s.	d.
49¾ bushels of seed, at 5s. 4d.	13	3	11			
40 acres ploughing and harrowing, at 12s.	21	0	0			
8 days work with drill, at 7s. 6d.	3	0	0			
4 carts two days leading weeds, at 5s	2	0	0			
24 women cutting weeds, at 9d.	0	18	0			
				43	1	11
141 days ploughing and harrowing, at 5s.	35	5	0			
435 days work of women weeding, at 9d.	16	6	3			
45 days work of men, at 2s.	4	10	0			
				56	1	3
168 days work of women cutting, at 1s. 3d.	10	10	0			
30 men's days work, at 2s.	3	0	0			
66 women's days work, binding, at 1s. 3d.	4	2	6			
22 men's ditto, making bands, &c. at 2s.	2	4	0			
				19	16	6
27 men and horses leading the beans off the ground, at 5s.	6	15	0			
18 women's days work, at 9d.	1	2	6			
Stacking and leading the beans	7	15	0			
				15	2	6
				£134	12	2

Further expenses after the crop of beans was cut.

	£.	s.	d.	£.	s.	d.
Twice ploughing and harrowing 40 acres, at 12s.	48	0	0			
Ditto 6 acres a third time, at 12s.	3	12	0			
2 carts 6 days, leading off weeds and stones, at 5s.	3	0	0			
48 women picking, at 9d.	1	16	0			
10 men ditto, at 2s.	1	0	0			
				57	8	0

Cultivation of beans and wheat, &c.

Value of crop 628 bushels, at 5s. 4d.				£167	9	4
Expense of sowing, cleaning, and reaping the beans,	£134	12	2			
Had the wheat been then sown, the balance in favour of the crop would have been				75	12	0
				£243	1	4
By further expenses as above	57	8	0			
	192	0	2			
Balance in favour of the green crop, giving credit for the expense of the fallow	51	1	2			
	£243	1	4			

The appearance of the wheat is most promising. It is my intention to take another crop of beans, which will most completely clean the ground, then give a second dressing of from 20 to 30 cart-loads of compost, and sow it with wheat and seeds in the spring.

Should farther information be requisite, I shall be happy to give it.

I am, Sir,

Your obedient servant,

I. C. CURWEN.

Workington Hall, March 20, 1804.

Charles Taylor, Esq.

Dear Sir,

An opportunity offering by which I can send you a sample of my beans for the inspection of the Society, I think

think it more advisable than waiting till the meeting of Parliament; should it occur to you that any further information is requisite, I will be much obliged to you to acquaint me with it. I think I may, without arrogating too much, say, the manner in which the crop was worked and got into the ground, and its present appearance, is not inferior to any thing which has been done in any part of the kingdom. The accounts of expense were kept with great care and attention. I shall be highly gratified in being successful in my application for the medal. Should any information be wished by the Committee, my friend, Mr. Greene, of Bedford-square, would willingly attend, as he expressed great pleasure at what he saw whilst we were putting in the crop. It has drawn the attention of the farmers in the neighbourhood; and when I come over it again, I hope they will be sensible of the advantages resulting from the plan. I am this winter trying an experiment in feeding milch cows, and selling the milk to the poor, who have hitherto been extremely ill supplied. I conceive, by feeding the cows with green food and oil-cake, I can furnish the milk as cheap, and with as much profit as in summer. I give each cow four stone of green food, at 1½d. per stone, four pounds of oil-cake at 1d. straw 2d. making the total one shilling. New milk is 2d. per quart—any thing above six quarts is profit. I have thirty cows, mostly heifers; these afford less milk; but I can dispose of them without loss in March or April, having no keeping in summer, or design to interfere with other farmers. I sell near two hundred quarts per day, besides my own consumption, farm-house, &c. &c. The cattle are in admirable order. I keep them in open sheds, and turn them out several hours every seasonable day. The crops here were in general good. I had an acre and three rood of carrots, which produced five thousand stone; the ground was by no means good; but they were sown upon ridges, gathered as high as possible, with a double mould-board plough, and kept well worked during the summer. My success will induce many trials. I give five pounds each day to my horses, instead of oats, which saves me sixty Winchester bushels per week, or £20. The Bishop of Llandaff

Cultivation of beans and wheat, &c.

Cultivation of beans and wheat, &c.

Llandaff is very busily employed planting a hundred acres, mostly with Larch; not to interfere with him, I wait till next year, when I shall plant between one and two hundred acres, lately purchased.

With great respect, I am,
Dear Sir,
Your obedient humble servant,
J. C. CURWEN.

Workington Hall, Nov. 20, 1804.
CHARLES TAYLOR, Esq.

DEAR SIR,

I wish to add to the communication I had the pleasure of making to you, respecting the culture of beans, that I have threshed out two stacks, and found the straw most admirable fodder. Horses are extremely fond of it; and I have, in no instance, found it to disagree with them, which I have understood to be frequently the case when the bean stands till it is quite withered. This advantage in favour of cutting the bean green had not occurred to me, and will add much to the value of the crop, and supply the place of oat straw, which is nearly of equal value with hay. I have not used any hay this season, but given bean and other straw with potatoes and corn, and find the horses in high condition. The experience of every year convinces me of the great saving in my plan of feeding, as well as its being the best food that horses can have for keeping them in condition and health. Lucerne and an equal quantity of corn will not keep the horses in the same condition as with potatoes. It is supposed this feed is not adapted to quick work: I can only say, I seldom travel less than eight miles per hour with my carriage-horses so fed, and I drove them thirty-five miles, a few days ago, in four hours and three quarters, and this without any injury or distress to them.

With respect, I am,
Dear Sir,
Your obedient humble servant,
J. C. CURWEN.

Workington Hall, Jan. 25, 1805.
CHARLES TAYLOR, Esq.

V.

On the Arrangement and mechanical Action of the Muscles of Fishes. By ANTHONY CARLISLE, *Esq. F.R.S. F.L.S.* *

IT was my intention to have continued my physiological inquiries on the phenomena of muscular motion, by a series of chemical experiments; and to have communicated the result, when duly matured, to the Royal Society. But an unexpected request, made at a late period, for the Lecture of the present year, obliges me to defer those researches, and to limit the investigation of the subject I have chosen. Introductory remarks.

The application of the motive organs of animals has already furnished examples of general utility by increasing our knowledge of mechanical powers; and the cultivation of this study promises still further improvement. Peculiar structure of fishes.

The muscles of fishes are of a very different construction from those of the other natural classes. The medium in which these animals reside, the form of their bodies, and the instruments employed for their progressive motion, give them a character peculiarly distinct from the rest of the creation. The frame-work of bones or cartilages, called the skeleton, is simple; the limbs are not formed for complicated motions, and the proportion of muscular flesh is remarkably large. The muscles of fishes have no tendinous chords, their insertions being always fleshy. There are, however, semi-transparent, pearly tendons placed between the plates of muscles, which give origin to a series of short muscular fibres passing nearly at right angles between the surfaces of the adjoining plates. Lewenhoeck † appears to have overlooked these tendons, and the numerous vessels, which he describes in the interstices of the muscular flakes, I have not been able to discern. Their skeleton is simple; muscles voluminous. Tendons.

The motion of a round shaped fish, independent of its fins, The motion of a fish

* Read before the Royal Society, Nov. 1805, being the Croonian Lecture.

† Phil. Trans. Vol. XXXI. p. 190.

fins, is simple; and as it is chiefly effected by the lateral flexure of the spine and tail, upon which the great mass of its muscular flesh is employed, whilst the fins are moved by small muscles, and those, from their position, comparatively but of little power, I shall only describe in detail the arrangement and application of those masses, which constitute the principal moving organs.

— explained from the structure of the cod.

For this purpose a well known fish, the cod*, has been selected as a standard of comparison for the muscles of other fishes, there being a conspicuous resemblance among them all.

The side fins and back fins regulate position, &c.

The pairs of fins have been considered as analogous to feet, but they are only employed for the purposes of turning, stopping, altering the position of the fish toward the horizon, and for keeping the back upwards. The single fins appear to prevent the rolling of the body, whilst the tail is employed to impel it forward.

Manner in which the fins act.

Each of those fins, which are in pairs, is capable of four motions, viz. of flexion and extension, like oars, and of expanding the rays, and closing them.

The extension of the whole fin is performed by a single radiated muscle, which is often supplied with red blood: the antagonist is of a similar character. The greater power of the extensor muscle (Vide Plate V. *a*, *a.*) shews how strongly it is required to act when employed to stop suddenly the progressive motion. A series of intervening muscles expand and close the rays.

In the act of extending the fin the interosseal muscles are passive. It is advanced forward edgeways and closed; but during its flexion, the rays are expanded, striking the water with its broadest surface: this action assists the tail in turning the fish. In the effort to stop, these fins are strongly retained at right angles with the body, by the force of the extensor muscles, the rays are expanded, and the effect is assisted by the tail turuing laterally with its broadest surface forward.

The single fins, for the expansion and contraction of their rays, are furnished with two sets of muscles; one of which is situated at their roots, and lies oblique;

* Gadus Morhua of Linnæus.

(*bbbbb*) the other, parallel with the spines, to which the rays are articulated (*cc*). The fin has also a lateral motion, by which it is occasionally drawn out of a straight line; and by the co-operation of these muscles on both sides, it is kept steady whilst the body of the fish is turned oblique in swift motion, or in eddies. When placed near the tail, the single fins seem also to aid the effect of that instrument by increasing its breadth.

Explanation of the action of the tail.

The tail is the principal organ of progressive motion, and its actions are performed by the great mass of lateral muscles. There are a series of short muscles for the purpose of changing the figure of the tail fin, which arise from the spine and *coccyx*, and are attached to the rays immediately beyond their joints: (*dd*): their action is to expand the rays, and by partial contractions to alter the lateral position of the fin. Slender muscles are placed between the several rays, (*ee*,) whose office is to converge them previous to the stroke of the tail.

The muscles situated on the head are those, which act on the *membrana branchiostega*, the under jaw, *os hyoides*, *fauces*, and the globe of the eye.

Experiments on the action of fish deprived of their fins.

In order to determine the effect of the fins on the motions of fishes, a number of living dace*, of an equal size, were put into a large vessel of water. The pectoral fins of one of these fishes were cut off, and it was replaced with the others. Its progressive motion was not at all impeded; but the head inclined downward, and when it attempted to ascend, the effort was accomplished with difficulty.

The pectoral and abdominal fins were then removed from a second fish. It remained at the bottom of the vessel, and could not be made to ascend. Its progressive motion was not perceptibly more slow; but when the tail acted, the body shewed a tendency to roll, and the single fins were widely expanded, as if to counteract this effect.

From a third fish, the single fins were taken off. This produced an evident tendency to turn round, and the pectoral fins were kept constantly extended to obviate that motion.

* Cyprinus leuciscus.

From a fourth fish, the pectoral and abdominal fins were cut off on one side, and it immediately lost the power of keeping the back upwards. The single fins were expanded, but the fish swam obliquely on its side with the remaining pectoral and abdominal fins downwards.

From a fifth fish all the fins were removed. Its back was kept in a vertical position, whilst at rest, by the expansion of the tail, but it rolled half round at every attempt to move.

From a sixth fish, the tail was cut off close to the body. Its progressive motion was considerably impeded, and the flexions of the spine were much increased during the endeavour to advance: but neither the pectoral nor abdominal fins seemed to be more actively employed.

From a seventh fish, all the fins and the tail were removed. It remained almost without motion, floating near the surface of the water, with its belly upward.

These experiments were repeated on the roach*, the gudgeon†, and the minnow‡, with similar results.

Differences between the texture of the muscles of fish and other animals.

The muscles of fishes differ materially in their texture from those of other animals: they are apparently more homogeneous, their fibres are not so much fasciculated, but run more parallel to each other, and are always comparatively shorter. They become corrugated at the temperature of 156° of Fahrenheit, when their tendinous and ligamentous attachments are dissolved, and their serous juices coagulated. Under those circumstances the muscles lose their transparency, and the lateral cohesion of their fibres is lessened.

Mechanical arrangement and physiology of the lateral muscles.

But the mechanical arrangement and physiology of the lateral muscles of the body of fishes constitute my present object. These parts have already been described in a general way by Professor Camper, M. Vicq-d-Azyr, and M. Cuvier, to whom I am indebted for much useful information. They have been denominated *couches musculaires*, by M. Vicq-d-Azyr§, and *muscles laterals* by M. Cuvier‖. The term used by M. Cuvier seems very

* Cyprinus rutilus. † Cyprinus gobio. ‡ Cyprinus phoxinus.

§ Mem. étrangers de l'Académ. des Sci. de Paris. Tom. VII. p. 18 et 223.

‖ Leçons d'Anatomie comparée. Vol. I. p. 196.

appropriate

appropriate for the general division or class. But, as the flakes are arranged in distinct longitudinal rows, these rows must be considered as orders. And, as *couches* appears objectionable, I shall adopt *series* in its stead; distinguishing each by a word referring to its situation in the animal, viz. the dorsal, vertebral, abdominal, and ventral series.

These series are composed of thin masses of muscle, or, as they are commonly called, flakes; which for the most part are thicker upon their outward edges, and become wedge-shaped toward their interior attachments. Each series is separated from the next adjoining by a membranous partition, which is most apparent between the vertebral and abdominal series. They are disposed in flakes or series.

The dorsal series (*ff*) arises from the back of the head. In its course it is terminated on the upper edge by the bones, which support the single fins, and a membranous *septum*: at this part the flakes are thin. Its lower margin is bounded by the vertebral series, where the flakes become gradually thicker. The first flake is composed of longer fibres than the rest, and possesses more red blood. Those succeeding it range obliquely backwards. They are all joined together by cellular membrane, and shining fasciæ, which resemble the tendinous expansions in quadrupeds. Particular description.

Toward the middle of the fish the flakes are thicker, and stand more perpendicular to the surface, becoming oblique and thin as they approach the tail; whilst the intervening fasciæ are most dense at each extremity. This series consists of forty-five flakes, a number corresponding with that of the spinous processes to which they are attached, and which does not vary with the growth of the fish.

The muscular fibres constituting each flake, run nearly at right angles with its anterior and posterior surfaces, and parellel to the length and surface of the fish; except that their posterior extremities incline somewhat inwards.

As the skull affords the ultimate fixed attachment of this series, and its moveable insertions are on the vertebræ, and the tail, it follows, that its combined action is to bend the whole body and tail towards one side; or, if

Particular description of the muscles of fishes.

the flakes contract partially, to give it a serpentine motion. To produce these effects all the other series cooperate.

The superior external edges of the flakes of the vertebral series (*gg*) form acute angles with the inferior external edges of those of the dorsal series, the apices of which point toward the tail: the flakes are larger, but their number is the same. The lower margin of this series is bounded by the central membranous partition, which has already been noticed to be more conspicuous than the other longitudinal divisions, and it apparently admits of greater motion.

The abdominal series (*hh*) is composed of flakes similar to the preceding. They range toward the tail, forming an angle with those of the vertebral series, the apex of which is presented toward the head. They are attached internally to the transverse and inferior spinous processes of the vertebræ. The ribs are placed in the line of the centre partition, and lie between the flakes. This series arises from a bone which borders the opening for the gills, and the pectoral fin, with its scapula and muscles, is situated between its foremost flakes. Wherever this series encloses the viscera, its flakes are shallow, and their thickness internally is not much less than at their external superficies.

Lastly, the flakes of the ventral series (*ii*) form acute angles with the abdominal flakes, the points of which incline to the tail. It is attached anteriorly to the *os hyoides*, and the bones of the lower jaw. In its course it is bounded above by the abdominal series, and below by a membranous *septum*, within which the inferior single fins arise. The flakes, that cover the viscera, are shallow; and they lie more oblique as they approach the tail. Both this, and the last described series, have their muscular fibres arranged according to the length and figure of the fish.

Three large superficial nerves (*kk*) passing longitudinally from the head to the tail, in the course of the membranous partitions, give off fibrils at right angles, which bend inwards between each of the muscular flakes. A larger set of nerves are sent from the *medulla spinalis*, one between

between each flake, the branches of which seem to enter without ramifying there. Another small nerve passing from the head, and running deep-seated, and close to the dorsal spines, crosses and unites with each of the spinal fibrils, and at the junction a remarkable body appears; it is a loose transparent vesicle, about the size of a millet-seed, containing a white substance like the carbonate of lime found in the intercostal ganglions of frogs. This vesicle is included within the sheath of the nerve.

Particular description of the muscles of fishes

The coats of the blood-vessels are of a delicate texture, and easily ruptured. In order, therefore, to secure them from being injured by the violent and sudden actions of the muscles, the principal trunks both of the arteries and veins are inclosed in osseous canals, formed by the bases of the superior and inferior spinous processes; and their first ramifications lie within grooves in the spines. As they pass out to supply the muscles, their branches are immediately subdivided, so that a considerable vessel soon becomes extremely minute.

Fish move with surprising velocity; nearly equal to that of birds.

The rate, at which many fishes move through a medium so dense as water, is very remarkable; their velocity being scarcely surpassed by the flight of the swiftest birds: and although the large proportion of muscles, and their advantageous application, may partly account for the phenomenon, yet the power would be inadequate to the effect, if it were not suddenly enforced; as is evident from the slow progress of eels, and such fishes as are incapable from their length and flexibility, of giving a sudden lateral stroke.

This extreme force is soon exhausted.

But the quickness and force of action in the muscles of fishes are counterpoised by the short duration of their powers. Those accustomed to the diversion of angling, are aware how speedily the strength of a fish is exhausted, for if, when hooked, it be kept in constant action, it soon loses even the ability to preserve its balance, and turns upon its side, fatigued and incapable of motion. This has been vulgularly attributed to drowning, in consequence of the mouth being closed upon the hook; but the same effects take place when the hook is fastened to the side, or tail. This prostration of strength may depend partly on fear, and partly on interrupted

terrupted respiration, since fishes, when swimming rapidly, keep the *membranæ branchiostigæ* closed, and when nearly exhausted, act violently with their gills.

The structure of their muscles forms a contrast to slow-moving animals.

The shortness of the muscular fibres, and the multiplied ramifications of the blood vessels, are probably peculiar adaptations for the purpose of gaining velocity of action, which seems to be invariably connected with a very limited duration of it. Such examples form an obvious contrast with the muscular structure of slow-moving animals, and with those partial arrangements where unusual continuance of action is concomitant.

These doctrines further illustrated.

Since my former communications on the subject of cylindrical arteries*, another instance of their supplying slow-moving muscles, which are capable of long continued action, has been pointed out to me by Mr. Macartney. It is in the muscles, which act upon the feet and toes of many birds, and seems to be an adaptation for the long exertion of those muscles while they sleep, and also when they alternately retract one foot under the feathers to preserve it from the effects of cold.

The muscles of the human body, which perform the most sudden actions, have their masses of fibres subdivided by transverse tendons, or are arranged in a penniform direction. The semi-tendinosus, and semi-membranosus of the thigh are thus constructed; the former having its fleshy belly divided by a narrow *fascia*, and the fibres of the latter being ranged in a half-penniform manner. The *recti abdominis* are also divided into short masses by transverse tendons, and all these muscles are conjointly employed in the action of leaping.

Perhaps these observations may indicate the reason for that diversity in the lengths of various muscles, which act together; thus, organs of velocity are joined with those of power, and mutually co-operate to produce a simultaneous effect.

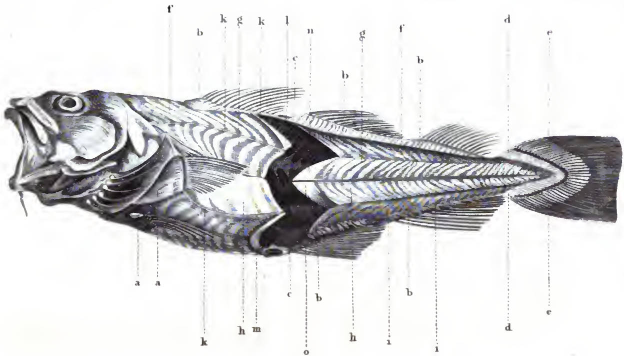
DESCRIPTION OF PLATE.

Explanation of the engraving. Pl. V.

The drawing was made from a cod which had been coagulated by heat, in a case of plaister of Paris, the skin

* Phil. Trans. 1800, p. 98.—Also 1804, p. 17.

being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eing taken away, and an equal portion of the flakes arefully removed from each series, to exhibit their sevel directions. The subject was reduced to the present ze by accurate measurements.

aa, Muscles which extend the pectoral and jugular ns.

bbbbb, Oblique muscles, which erect the rays of the ngle fins.

cc, Muscles which depress the rays.

dd, Muscles which extend the rays of the tail.

ee, Interosseal muscles, which close the rays.

ff, The dorsal series of muscular flakes.

gg, The vertebral series.

hh, The abdominal series.

ii, The ventral series.

kkk, Three superficial nerves which run longitudinally etween the series of flakes.

l, Posterior surface of a dorsal flake.

m, Posterior surface of an abdominal flake.

n, Anterior surface of a vertebral flake.

o, Anterior surface of an abdominal flake.

The middle portion of the fish from whence the flakes have been removed, shews the several directions of them, and also their different thicknesses. The spine appears in the chasm.

VI.

On the Use and Abuse of Popular Sports and Exercises, resembling those of the Greeks and Romans, as a National Object. By SAMUEL ARGENT BARDSLEY, M. D.

[From the Memoirs of the Manchester Society, Vol. I.]

HUMAN nature is so constituted as to require both bodily and mental recreation. This instinctive propensity to amusement in man, is sufficiently proved by the universality of the appetite, in every stage of life, under every variety of clime, and constitution of government. But the regulation of this natural propensity differs greatly

Recreation or amusement

ly according to the circumstances under which he is placed. The recreations and sports of mankind are therefore diversified by the influence of moral, political and physical causes. The means of gratification are various and complex: the end simple and uniform. To escape from the sensations which may be induced by too great or too little exertion of body or mind, and to enjoy the pleasure which sympathy extracts from the varied intercourse with fellow man, give rise to that fondness for public diversions and sportive contests, so conspicuously displayed in the history of mankind.—The influence of physical causes, in regulating the nature of these diversions, may be readily conceived.

—is sought by man in order to change his state of re-action.

Influence of physical causes affect our amusements. Activity is seen in a cold climate, indolence in a hot one.

The hardy, strenuous and active amusements of the inhabitants of the temperate and frigid zones, would depress and exhaust, rather than enliven and invigorate, the residents of a torrid clime. Hence the supreme delight of the Asiatic consists in the enjoyment of those pleasures which are purchased with little fatigue of body, or agitation of mind. To inhale the grateful fumes of his pipe, and to foil his adversary in the stratagems of chess, or other sedentary games, constitute the principal part of his amusements.

Moral causes act likewise.

Although physical causes necessarily circumscribe the sphere of man's active pursuits, yet they have much less controul than those of a moral and political kind. Man is endued above all other animals with a frame and constitution which can adapt itself to every diversity of clime and change of temperature. He can, in a measure, subdue physical obstacles, when powerfully stimulated by moral and political causes.—The savage, compelled to hunt his prey for food, has little leisure to cultivate his intellectual taste and powers. If not exposed to danger from hostile neighbours, his recreations are mostly of a negative kind.—He is happy when idle and at ease. But if he be stimulated by the prospect of war, all his amusements tend to accomplish him for carrying on successfully his military exploits. His songs are praises of the heroes of his nation; and his dances are connected with martial discipline. The public shews and festivals of

of his country are, almost without exception, of the character of savage war*.

According to the degree of civilization will the public sports and amusements of a people partake more or less of the mixed character of corporeal and mental recreation. A display of the arts which refine and gladden life, can only flourish where the condition of man has been long meliorated by the enjoyment of moral and political advantages.—Indeed the kind and nature of the popular sports and exhibitions of a people, whether just emerging from barbarism, or passing through the various stages of improvement, or arrived at the highest pitch of refinement, serve to measure, as by a scale, the different degrees of their advancement to the acme of civilization. The two most powerful and celebrated nations of antiquity, Greece and Rome, afford ample proofs of the truth of this remark. The shews and public sports of each of these nations, while they issued from their character and manners, operated on this very character and manners, and rendered them more ardent and permanent. This connection between the character of a people and their sports, was forcibly impressed on their legislators and rulers. Their public games were instituted for other purposes than mere amusement and relaxation. They were rendered subservient in Greece to the noblest views of legislative policy. Intimately connected with the whole system of government, whether civil, military, or religious, they had a moral as well as a political tendency. To promote ardor, emulation, friendship, patriotism, and all the animated principles and connections of active life, the Olympic and other solemn festivals were instituted. In

Civilization encourages mental recreation

Whence the state of barbarism or refinement may be measured from the public sports.

Legislators have often directed the public sports to moral and political objects.

* The savage tribes of America furnish various proofs of the truth of this remark.——Likewise in Collins' account of the natives of New Holland, there is a curious illustration of the propensity of a rude and savage people to those amusements which are adapted to their peculiar situation.

Indeed the singular and ludicrous ceremony of initiating youth into the rank of warriors, at the celebration of their military exercises and games, is a striking instance of that disposition to amusement, which even the most savage and wretched state of life cannot eradicate.

order to investigate some of the moral and political effects of these popular sports and public games, which contributed so largely in raising the Greeks and Romans to a height of unparaleled grandeur, it will be necessary to examine the foundation of a system, which, in some respects, when freed from its worse abuses, particularly those which the more ferocious character of Rome introduced, may not illaudably nor unusefully be imitated by the most civilized nations.

The ancient Greeks directed their sports to afford pleasure and to give agility and personal power.

Though it may, perhaps, be admitted, that the difference in the state of knowledge and general policy, in the ancient and modern world, will not admit of a close approximation in the system of their public sports and amusements; yet the principles to which the Greeks directed their attention in controuling popular amusements deserve the limited imitation of every free and enlightened people. For, their aim was to direct to innocent and useful objects two of the most powerful principles of the human breast;—the love of pleasure and the love of action. Hence arose the institution of the * gymnastic exercises, which formed the principal part of all the solemn games. The gymnastic art consisted in the performance of bodily exercises calculated for defence, health and diversion.

The athletic sports are practised by all nations.

That branch of these exercises, called the athletic or sportive, must be considered as coeval with the formation of society †. The five ‡ gymnic exercises, so

* Lycon, according to Pliny, first instituted the gymnastic games in Arcadia, whence they were extended throughout Greece and successively contributed to the highest gratification of both the Greeks and Romans, in their private schools and public solemnities.

They were performed in the *Gymnasium*, where not only youth were instructed in these exercises, but also the philosophers taught their different doctrines.—The Palæstra, which formed a part of the building, was the school for the gymnic exercises.

† In almost every island of the Great Pacific Ocean, we find a similarity, more or less striking, in the athletic and warlike exercises of the natives, with those practised in Greece.

‡ These five exercises were called Pentathlum by the Greeks, and Quinpertium by the Romans. They consisted of leaping, running, throwing the Discus, darting the javelin, and wrestling; but instead of darting the javelin, others mention boxing. The last exercise

so accurately described by Homer, Pindar, Sophocles, and Pausanius, formed the principal branch of the education of youth.

How the Greeks were educated.

To be enabled to excel in the performance of these, they were trained with the greatest care; and every means was employed to excite powerful emulation. Their object was, to recreate and strengthen the body, as well as fortify and exalt the mind. For, the firm organization acquired by perpetual exercise, counteracted the propensity to vicious indulgence, which a voluptuous climate naturally inspires.

Animal vigour gives courage.

They likewise infused a courage depending on animal strength and vigour, which was excited to the highest pitch among this warlike people *. Besides, the ambition of honest fame (the sure † reward of excellence in these sports and contests) taught them to controul the appetites of the body by the affections of the soul.

—and renders men capable of defending their country by military art.

But the chief aim and end of the institution of athletic gymnastics among the more warlike states of Greece, were, perfection in the military character. Their philosophers inculcated this doctrine by their precepts and example.—Plato, in his book of laws, after having viewed the high importance of acquiring bodily force and agility, adds, "a well governed common-wealth, instead of prohibiting the profession of the athletic, should, on the contrary, propose prizes for all who excel in those exercises, which tend to encourage the military art."—And, perhaps no better plan could have been contrived to foster a warlike spirit amongst a people devoted to military enterprize, than the training of youth in these hardy and laborious exercises, and in proclaiming rewards for those

ercise was combined with wrestling; and then took the name of Pancratium.——See Hieronymus Mercurialis, de arte gymnasticâ —and Potter's Archœologia.

* Hac arte, Pollux & vagus Hercules
Innixus, arces attigit igneas.—Hor.
" Thus mounted to the towers above,
" The vagrant hero, son of Jove."

† Such as gained victories in any of these games, especially the olympic, were universally honoured, and almost adored.——See Plutarch's Sympos. lib. II. Quest. VI. and Potter's Archæol.

 who

Mere strength or animal courage is attended by ferocity.

who excelled in their public exhibition. If man were only destined to conquer and triumph over the weaker and less valiant of his race—if the lust of dominion were the only appetite worthy of gratification, then the cultivation of bodily prowess and ferocious courage would properly form the business, as well as pleasure of life. But man has a nobler part to act in society; and enjoyments more pure, lasting, and better fitted to the dignity and character of his nature, become necessary to his well being in an advanced stage of civilization. It may readily be conceived, that those arts which sooth and embellish human existence, and which depend on the cultivation of feeling and of taste, would be neglected by the Greeks, when only bodily strength, activity, and address could carry off the palm of victory. In the distracted state of the first settlers in Greece, when the bodily energies were constantly in action, courage and personal strength decided the day in most of their military conflicts. Hence courage became associated with every idea of patriotism, honour, and virtue. It is the opinion of Aristotle, "That the nations, most attentive to the formation of the body, strive to give it too athletic a habit, which injures the beauty of the shape, and stints the growth of the person. The Lacedemonians avoid this error; yet, by imposing excessive labour on the body, they engender ferocity in the mind, thinking this conducive to martial spirit. But mere warlike courage, taken separately by itself, is a doubtful and defective quality, and, cultivated too assiduously by the hardening discipline of toils and struggles, will degrade and debase the *man*, blunt his faculties, narrow his soul, and render him as bad a soldier as he is a contemptible citizen *." This necessity of rendering the gymnastic art subservient to nobler pursuits, was felt and acted upon by the Athenians, and other polished states of Greece.

Men who have only animal strength and ardour will not be good soldiers.

The Greeks cultivated poetry and music as part of their public games.

The cultivation of poetry and music was encouraged by bestowing the highest honours and rewards on those who excelled in these delighful arts at the celebration of all the public games. To such a happy combination of

* Gillies's Aristot. polit. p. 250.

mental

mental with corporeal excellencies, cherished and displayed under the most pompous and fascinating appearances in their popular diversions and solemn festivals, may the splendid achievements of this distinguished people be attributed *. Considered in the light of affording amusement, exciting generous emulation, and of creating robust and hardy citizens, endowed with energy to resist slavery at home, and enemies from abroad, the gymnastic exercises, with some exceptions, and under proper regulations, are worthy of the admiration and imitation of all free and civilized states. But there was another kind of popular sport, common to the less polished states of Greece, and which has been practised by mankind, not only in the rude and barbarous, but (to the disgrace of humanity) in the most advanced and polished period of civilized life. This amusement depended on the contests of ferocious animals, whose natural antipathies were made use of, and designedly inflamed to gratify a depraved and barbarous taste.—"They delight," says Lucian, (speaking of the Greeks,) "to behold the combats of bold and generous animals, and their own contentions are still more animated."—The savage ferocity inspired by the frequent repetition of such barbarous exhibitions, accounts in some measure for the conduct of the Ephori of Sparta, who, when they declared war against the Helots, ordered that the young bull-dogs should be employed in

Public sports, consisting in the contests of ferocious animals; which are depraved and barbarous.

* Montesquieu is of opinion, that the want of employment for the majority of the citizens, compelled the Greeks to become a society of athletic and military combatants; for, he observes, "they were not permitted to follow the ordinary occupations of agriculture, commerce, and the baser arts; and they were forbidden to be idle; consequently, their only resource was in the gymnastic and military exercises." But this assertion is contradicted by the practice of some of the Grecian states. We know that in Athens commerce was highly esteemed and successfully cultivated. This writer must therefore be understood in a restrictive and qualified sense, when he says, "Il faut donc regarder les Grecs comme une societé d'athletes & des combattans."—Montesquieu de l'esprit de loix. liv. IV. chap. VIII.

The Pancratium, in which the antagonists voluntarily threw themselves on the ground, and annoyed each other by pinching, biting, scratching, and every kind of savage attack, ought not to be endured in a civilized country.

worrying

These enormities extended to all ranks, and even to women.

sary to gratify their depraved appetites by the exhibition of human butchery and sacrifice. So lost to every spark of decency and humanity were this infatuated and ferocious people, that the highest ranks of society gloried in voluntarily taking a part in these encounters: and even the softer sex, throwing aside every trait of amiable modesty and timidity, were ambitious of displaying their personal courage in these savage contests. This conduct did not escape the lash of the Roman satyrist.

" Cum —— Mævia Tuscum,"
" Figat aprum, & nuda teneat venabula mammâ."

Persons of every age, sex, and condition attended these barbarous sports. The intoxication of the populace, from frequent gratification, arose to such a pitch, that streams of blood flowed annually from several hundreds, perhaps thousands, of the wretched gladiators, throughout the various cities of the empire. When the people had been so far steeped in blood as to prefer beyond any other these sanguinary combats, all the candidates for high offices bribed their favour, by outvying each * other in the number and pomp of these impious shews. Even the most powerful and enlightened minds among the Romans were tainted by the contagious influence of custom and the strength of national prejudice: Cicero, the humane and dignified statesman and philosopher, very faintly, if at all, disapproves of the excessive fondness of the people for this abominable exhibition in his time; and plainly expresses his approbation of the practice as antiently conducted. His words are, "crudele gladiatorum spectaculum & inhumanum nonnullis videri solet; & haud scio an ita sit, ut nunc fit: cum verò sontes ferro depugnabant, auribus fortasse multæ, oculis quidem nulla poterat

These habitual cruelties vitiated even the minds of their philosophers.

* Julius Cæsar, in his Edileship, presented three hundred and twenty pair of gladiators—and Trajan, as averse from cruelty as the former, brought out 1000 pair of gladiators during a solemnity of 123 days. But the sanguinary hero enlisted 400 senators and 600 knights (if there be not a corruption of the text of Suetonius, the historian) as gladiators, at a celebration of the Circensian games.—See Gibbon's History of the Decline and Fall of the Roman Empire.

esse

esse fortior contra dolorem & mortem disciplina."—"The shews of gladiators to some persons may seem barbarous and inhuman: and I don't know as the case now stands that the censure is unjust:—But when only guilty persons were the combatants, the ear might receive better instruction—it is impossible, however, that any lesson to the eye can better fortify the mind against the assaults of grief and death." A ridiculous and inhuman assertion (an eloquent historian exclaims) admirably confuted by the bravery of antient Greece and modern Europe. *Absurd and inhuman argument.*

Indeed so little was the practice connected with military ardour and true courage, that before its establishment the Romans were, perhaps, more distinguished for bravery, steadiness of discipline, and contempt of death, than at any subsequent period of their history. *The Romans did not improve in military spirit;—*

It is, however, certain, that in proportion to the frequency and extent of these bloody exhibitions, did the military valour and discipline of the Romans sink into a state of degradation and contempt. "After subsisting a period of 600 years" (according to the remark of Gibbon), "Honorius gave the final blow to this inveterate abuse, which degraded a civilized nation below the condition of savage cannibals." *—but became degraded in these times of cruelty.*

Rome justly suffered moral and political evils from fostering such inhuman propensities: her existence was more than once at stake by the insurrection of the wretched and despairing victims of her barbarity. Besides, the corruption of the populace, through the medium of these diversions, was no difficult task to the powerful and wealthy. When man has been taught to subdue the humane feelings of his nature, he contracts an indifference to the purer and nobler virtues which fit him for discharging the duties of a good citizen. Indeed every habit that wears out the sympathizing sensibility of the heart, proportionably disqualifies man from exercising the pleasing duties and tender charities, connected with public and domestic life. *Danger of insurrection.*

It would appear from this hasty sketch that the popular games and exercises of the Greeks when compared with those of the Romans, were better calculated to promote the social as well as individual welfare of mankind. *The Grecian sports had good effects: the Roman, the contrary.*

The Grecian sports fortified the body and disciplined the mind, without injuring the one or brutalizing the other.

Universal depravity accompanied the latter;

Indeed the superior wisdom of Grecian policy rendered the public diversions subservient to the interests of the state as well as to the happiness of the people. The Roman government did not always neglect this branch of policy. For, their sports, in the early and rude state of the nation, were adapted to the circumstances in which the people were placed. But incessantly harassed themselves, or employed in harassing others, they had neither leisure nor inclination to cultivate those arts which contribute to liberal amusement: ever occupied with warfare, all their amusements had a warlike tendency. The contests of savage animals and the conflicts of gladiators, suited alike the ferocious manners of the populace and the political views of their rulers. When the empire had subdued more polished nations, it might have been expected, that its amusements would have assumed a different spirit and complexion. But the habits of the people were too deeply rooted and depraved to be easily changed—And, indeed, so far were their rulers from wishing to accomplish this reformation, that, from corrupt and selfish views, they studiously excited the propensities of the people toward degrading and inhuman shews, by administering constant food for these savage enjoyments.

— which was not changed by intercourse with more polished nations.

Modern civilization has bettered the condition and manners of society;

In the progress of civilization, since the downfal of the Roman empire, great and important changes have taken place in Europe, with respect to religious, political, and civil institutions. The melioration of the condition of man in his social and domestic state, and the general refinement of his character and manners, have been the happy result of these moral and political revolutions. Yet still there remain sufficient vestiges of antient barbarity to throw a dark shade on the present state of improved civilization. The cruel sports still so highly relished in many parts of modern Europe, and which bear so near a resemblance to the savage contests of the Circus, exhibit lasting and disgraceful proofs of the relics of antient barbarism. Our own country has been but too justly stigmatized, even by her less polished neighbours, for

— but barbarity still remains;

for the devotion of the lower ranks of the people to those amusements which are derived from the sufferings of the brute creation.

Although the resemblance (whether it be original or imitative is of little importance) between the cruel diversions of England and of Rome, may be considered a subject of just regret; yet the similarity in some of the manly exercises and hardy sports, practised by the two nations, cannot but claim our warm and just admiration.

Especially amongst us; but they are mostly athletic.

If we have retained more of the barbarous sports of antiquity than the rest of Europe, there is the merit due to us of having more extensively adopted and practised those amusements and exercises, which inure the body to labour and fatigue, and inspire the mind with courage and emulation. In treating on the general character and spirit of some of the sports and exercises of the people of England, it will not be necessary to enter into particular detail. It is only proposed to hint at those of a popular nature, and which seem to be interwoven with the customs and manners of the mass of the people. They may be comprised under two heads.

1st. The sports which are derived from the animal creation.

2d. The amusements which depend upon bodily exercises and personal contests.

British sports, contests of animals.

I. It cannot be denied, that mankind, at every period of society and under every diversity of country and government, have rendered the animal race subservient to their wanton and cruel sports. But the universality and antiquity of a practice, founded on inhumanity and impolicy, are inadequate to sanction its utility and continuance. If it can be shewn that barbarous sports tend to brutalize the human character, and are inconsistent with the manifest intentions of Providence; the argument derived from long custom and authority must fall to the ground. There is a sympathy implanted in our natures, which renders us feelingly alive to the pains and pleasures of our fellow-creatures, and is even extended to every part of the animal creation. Upon the due exercise of this principle depends great part of our social and individual happiness.—Whatever then has a tendency to

Cruelty to animals will destroy the general sympathy of man;

diminish the influence of this principle, ought carefully to be avoided. Now every single act of cruelty contributes its share toward the weakening or extinguishing the principle of sympathy; and by the repetition of such acts, according to the general laws of habit*, a disposition to cruelty is likely to be generated. If a child be early indulged in sportively tormenting *animals*, and this vicious propensity be suffered to grow up into a habit, his sensibility to *human* suffering will be proportionably diminished; insensibility will harden into brutality; and at length he will not be restrained from positive acts of cruelty toward his own species, whenever goaded by the feelings of interest or of passion. Hogarth, our great moral painter, has admirably illustrated the progress of cruelty in the human breast. The first stage of his hero's career is marked by sportive and wanton barbarity to animals. Upon this foundation crimes are soon erected; and at length grown callous to every social † and moral feeling, he closes his profligate career, by the perpetration of a deliberate and cruel murder. Another excellent judge of the human heart, Dr. Moore, has forcibly depicted the effects of wanton cruelty to the inferior crea-

— and render him callous to every proper feeling in society.

Illustrations by Hogarth and Dr. Moore.

* "The habitude which the people of this country (viz. Cape of Good Hope) necessarily acquire in witnessing instances of cruelty on human as well as brute creatures, cannot fail to produce a tendency to hardness of heart, and to stifle feelings of tenderness and benevolence. In fact, the rigour of justice is seldom softened with the balm of mercy."—See Barrow's Travels in Africa, Vol. II. p. 41.

† Such is the general impression on the mind of the power of habit to generate cruelty, that in most countries, those occupations which employ men in the destruction of animal life for the sustenance of human kind, are held in degradation and contempt. The lowest of the butchering tribe, in default of an executioner, is compelled to perform his functions in France and many other parts of the continent. There is an opinion prevailing in England, that butchers, and even surgeons, are equally disqualified, by the nature of their occupations, to sit upon juries, in trials affecting the lives of their fellow-subjects. This is probably a popular error; or, if true, yet a much more honourable reason may be assigned, why surgeons are not required to act in the capacity of jurors. Their office is to administer to the sufferings and calamities of their fellow-creatures—and it is fit they should every moment be disengaged and free to obey the summons to so humane a duty.

tion,

tion, in the character of Zeluco. The feelings of humanity became stifled in this monster's breast, from an early gratification of his caprice and passion in sporting with the torments of the animal race. It is likewise our duty as well as moral advantage—to refrain from all acts of wanton cruelty to the brute creation. The organs of sensation in all the inferior animals, are evidently adapted for receiving and transmitting impressions of pain and pleasure,—and although deprived of speech, their groans and cries are intelligible indications of their painful feelings. Nor are animals less capable of expressing signs of pleasure, as well as of suffering. This provision for the gratification of their several senses, is a sufficient proof of the intentions of the Creator. Like man, they were formed to feel and to enjoy. Here rests the foundation of their natural right to protection and humane treatment from mankind.

The system of nature requires that animals should be destroyed;

It cannot be inferred from this mode of reasoning, that animal life should in every instance be held sacred. The laws of nature and necessity demand from us the painful sacrifice.—Man must destroy life in order to live. Besides, we must consider that if man had subsisted only on vegetable food, the majority of the animal race which furnish his table would never have enjoyed life. Instead of increasing the breed of animals, he would have been compelled to destroy them to prevent a famine.—But barbarously, wantonly and deliberately, to torture and destroy animal life, is equally repugnant to humanity, duty and the best interests of mankind. Experience teaches us, that the common sense and feeling of mankind, condemn that man whose greatest delight seems to consist in bloody and barbarous sports.—Youth, it must be observed, commonly inflict pain on animals in mere sport without a due knowledge of the evil they commit. And the ignorant populace frequently err from the same cause. They are led to consider, but too often, from the connivance and even enconragement of their superiors in knowledge and station, that the animal race are equally indifferent to pleasure or pain; and only created for the purpose of gratifying the appetite, or contributing to the diversion of mankind.

—but not with the wanton infliction of torture.

If

Question. Whether all sufferings inflicted by man or animals are to be condemned?

If the question be asked,—" Whether all sports derived from animal suffering be entitled to equal condemnation?" The answer is decidedly in the negative.—For, although perhaps none can be completely justified, yet there is still a wide difference in the degree of moral and physical evil resulting from their practice. That class of diversions pursued for the benefit of health and exercise, where the enjoyment of pleasnre springs from the exertion of our active faculties, must not be compared with those depraved and cruel sports, which merely consist in the torture and destruction of the animal. In the present state of society, active diversions become almost necessary to the well being of the opulent and sedentary classes of mankind. Man cannot be happy withont occasional active employment. He pines in the lap of ease and pleasure, and requires the stimulus of animated exertion.—Hunting in all stages of society has therefore formed a principal share of the business and pleasure of man. But in this kingdom especially, a considerable portion of its inhabitants devote part of their time to the active and vigorous pursuits of the chace. And although it may be urged in favour of this exercise, that it invigorates the spirits, teaches men to despise enervating pleasure, and inures them patiently to sustain hunger, cold and fatigue; yet it cannot be denied, that it has a tendency, when too eagerly pursued, to blunt the sensibility,—to render the manners rude and coarse, and thus to degrade the dignity of the human character. The man of enlarged understanding, liberal notions and elegant manners, may occasionally call in the aid of the chace to relieve the fatigue of sedentary employment, or renovate the powers of nature, exhausted by mental exertion, without much apparent injury to his manners or morals;—but *frequently* to take pleasure in that, by which misery to animals is inflicted, if not absolutely vicious, is yet of no good tendency; it conduces neither to form the gentleman nor the man.

Apology for hunting.

If it be considered as too nice and fastidions a delicacy to impute blame to the practice of destroying animals for the purpose of health, exercise and recreation, it may, however, be allowed to call in question the policy and humanity

humanity of other diversions, once highly cherished, and still too much practised by the people of this country. Some of these national sports are sanctioned by the practice and encouragement of many persons distinguished for rank and talents.—That there should be found such abettors of the bloody and barbarous diversions of *cock-fighting* and *bull-baiting*, is both a subject for surprise and regret.—These two amusements seem to have survived the destruction of many other sports equally as unmeaning and barbarous; but that they should not have entirely yielded to the improved state of manners—or the interference of the laws, is a subject of just reproach to us by foreigners, and of deserved reprobation by the humane and reflecting of our own countrymen. The reciprocal influence of sports and manners on each other, may be shown from these and similar diversions, as practised in various periods of our history. A late ingenious and laborious writer* has described the ancient and modern diversions of the people of Great Britain, from the earliest authentic records to the present time.—This picture confirms the general truth of the position:—That as a nation improves in manners and civilization, it loses its high relish for inhuman and ferocious diversions. It is more than probable, that the sports derived from animal contests, such as bull-baiting, bear-baiting, and cock-fighting, are vestiges of Roman amusements introduced by that people into this conquered island. It is at least certain they were practised † in the early period of

Cock-fighting and bull-baiting condemned

* See Strutt's Diversions and Pastimes of the People of England.

† The jongleurs or jugglers, in the reign of Henry the 2d, made a profession of training bulls, bears, and even horses, for the purpose of baiting them with dogs.—The sport of fighting cocks in pitched battles, first appears on record in the same reign. During subsequent reigns this sport became general; and to the disgrace of our country was countenanced by royal favour during James the 1st and Charles the 2nd's reign. If the Romans set us the example in devising these sports, it must be confessed, we have "bettered the instruction." For to English refinement and ingenuity may be ascribed the noble invention of the Gaffle or Spur; by the aid of which, the gallant combatants of the cockpit mangle, torture and destroy each other; no doubt to the great satisfaction and delight of

of our history. During the military enthusiasm of the middle ages, while jousts and tournaments furnished amusement to the nobility and gentry, martial exercises constituted the chief diversions of the body of the people.

Science of defence. Prize fighters or English gladiators.

Hence arose the establishment of schools for teaching the "Noble science of defence," as it was called. These laid the foundation for professed gladiators, or prize-fighters.—The great prevalence of murder, robbing and every species of barbarity, in consequence of these proceedings, during the reign of Edward the First, compelled the government to issue an edict to suppress the schools as well the combats of prize-fighters.

During the reign of Henry the Seventh and Henry Eighth, these schools were revived in consequence of a supposed degeneracy in the military spirit of the people; and the baiting of animals at the same time became a favourite * diversion.

The bear garden.

The Bear-garden†, during the 16th and the early part of the 17th century, was the place of rendezvous for the highest as well as the lowest classes of society. The Tatler, when treating on the barbarous sports of this national circus, and the comments of foreigners on the subject, adds,

of admiring spectators. Another instance of our barbarous ingenuity must not be omitted. No other nation but the British has contrived to put in practice the *Battle-Royal*, and the *Welch-Main.*—In the former, the spectator may be gratified with the display of numbers of game-cocks, destroying each other at the same moment without order or distinction. In the latter, these courageous birds are doomed to destruction in a more regular, but not less certain manner. They fight in pairs, (suppose 16 in number) and the two last survivors are then matched against each other; so that out of 32 birds, 31 must be necessarily slaughtered.—See Pegge's Essay on the Archœlogia Britannica.

* Stephen Gossen, in the latter end of Henry 8th's reign, considers that our ancestors had entirely sunk into the lap of effeminacy, as may be proved by the following singularly quaint and alliterative style of abuse. "Our wrestling at arms is turned into wallowing in ladies' laps; our courage to cowardice; our running to riot; our bows into bowls; and our darts into dishes.

† Another common diversion, during the period of Queen Elizabeth and in the two following reigns, consisted in several persons at the same time scourging with whips, a blind-folded bear round the ring, whose sufferings and awkward attempts at revenge highly gratified the noble, as well as ignoble spectators.

adds, "I wish I knew how to answer the reproaches which are cast upon us, and to excuse the death of so many innocent cocks, dogs, bulls and bears, as have been set together by the ears, and died an untimely death only to create us sport." Bull-baiting was not confined within the limits of a bear-garden, but was universally practised on various occasions, in all the towns and villages throughout the kingdom. In many places the practice was sanctioned by law, and the bull-rings affixed to large stones driven into the earth remain to this day, as memorials of this legalized species of barbarity. The regular system of bull-baiting seems to have commenced with the reign of King John. Its general prevalence since that period, until within a few years, must have produced important effects on the manners and character of the people. The misery it has inflicted on the harmless and inoffensive brute, is a matter of no small regret and indignation with the humame and considerate part of mankind;—but the injury done to public morals and social huppiness, by an attachment to this degrading pastime, is still more to be deplored. Numbers of bulls were, and still continue to be, regularly trained and carried about from village to village, to enter the lists against dogs bred up for the purpose of the combat. To detail all the barbarities committed in these encounters would be a disgusting and tedious task. All the bad passions which spring up in ignorant and depraved minds are here set afloat. The torments and blood of the suffering beast, are purchased by money of his unfeeling master; and the owners of the dogs are not more gratified in gaining their sanguinary wagers, than in applauding the savage ferocity displayed by these animals. We cannot often appeal to the annals of bull-baiting;—but if they were regularly laid open, it is probable that many instances of a similar kind to the following might be held up as a lesson to the abettors of such diversions.—* "Some years ago at a bull-baiting in the North, a young man, confident of the courage of his dog, laid some trifling wager, that he would at separate times cut off all the four feet of his dog, and that after

Bull-baiting still continues

to degrade the public morals and disgrace the nation.

Detestable barbarities practised by the followers of this sport.

* See Bewick's Quadrupeds.—Article Dog.

 every

every amputation he would attack the bull. The cruel experiment was tried, and with success." Such detestable barbarity can only be exceeded by the following recital extracted from the public prints of 1799. At a bull-baiting in Staffordshire, after the animal had been baited by single dogs, he was attacked by numbers let loose at once upon him.—Having escaped from his tormentors, they again fastened him to the ring; and with a view either of gratifying their savage revenge, or of better securing their victim, they actually cut off his hoofs, and enjoyed the spectacle of his being worried to death on his bloody and mangled stumps. These facts speak more than a volume against the sophistic arguments of the advocates for exciting brave and manly courage by the exhibition of bloody and barbarous sports.

[To be concluded in our next.]

VII.

Remarkable Effect of the Effluvia from Ammonia Muriate of Platina on the Eyes, Nostrils, Throat, and Lungs, as in a Catarrh. In a Letter from An Occasional Correspondent.

To Mr. NICHOLSON.

SIR,

Effects not hitherto noticed

I DO not know that the following effects of the effluvia of precipitate of platina by muriate of ammonia, i. e. of ammonia muriate of platina, have been observed; and whether or no these are like those from the effluvia of ipecacuanha, in occasion, as asthmatical paroxysms, only to be considered as produced in particular constitutions, I must learn from the intelligent correspondence of your journal.

--on the system, by vapour from ammonia muriate of platina.

Every time I have had occasion to open a paper, or small parcel of the above precipitate, although I merely touched it with my fingers, or even when I did not touch it, but merely inspected it for a minute or two, I was in a few

few minutes affected with an uneasy sensation in my eyes, nostrils, throat and lungs, exacting a discharge of tears, sneezing, with running from the nose as in a catarrh, shortness of breath, attended by an itching and heat of the face with sometimes redness of it as from erythema. The last time I was affected, although I had not touched the precipitate, I experienced along with the above effects a slight disagreeable taste, and the dyspnœa continued after the catarrhal symptoms had vanished; which they do usually in about an hour. It may be proper just to mention also, that after leaving my laboratory for about two hours after the above effects came on, and by which time I was nearly recovered, as soon as I returned to the place where I opened the parcel, but did not again expose myself to it, the above symptoms were again brought on, although in a slight degree. I found wetting my face with cold water very serviceable in removing the erythema, and removing of course the heat.

I take for granted it is commonly known that by a similar exposure to the powder of ipecacuanha root, a fit of asthma is brought on in particular persons, although so rarely that such persons are considered to have what is called idiosyncrasy of constitution.

The diffusion through the air, manifested in the above cases of invisible and imponderable particles of matter, may serve to enable us to conceive the mode in which infectious matters are communicated.

I am,

Dear Sir,

With much regard,

An Occasional Correspondent.

Oct. 10th, 1806.

VIII.

Extract from a Letter of M. Proust, to M. Vauquelin, on Porcelain and on the Alimentary Use of Lichen Islandicus.*

Madrid, Dec. 22, 1805.

Sir,

Excellent pottery made from spuma maris.

I was visited by one of your pupils, M. Siquiera, an interesting and intelligent Portuguese young gentleman. We are going, to-morrow, to see the manufacture of porcelain under the direction of M. Sureda, who was brought up to this art in the manufactory of Sevres, and now makes a most beautiful porcelain, of a much harder texture than yours. This is not effected by means of kaolin, but with the *spuma maris*, a *siliceous magnesian stone*, found in the neighbourhood of Madrid; we shall send you some patterns which will astonish you. He covers his biscuit with feldspaths of Gallicia, which are very beautiful. The stone above mentioned would be excellent for the construction of chemical furnaces. When taken from the quarry, it is soft and admits of being cut like soap. Furnaces made of this stone are extremely light, and never undergo fusion, however intense the heat may be. Were such a fossil to be met with near Paris, we might do without the *rue mazarine*, (qu?). Besides magnesia, silex, and some particles of argil and lime, this stone contains a portion of potash, which contributes, not a little, to the superior qualities of the porcelain.

Lichen Islandicus; its value as food.

The following fact is perhaps no less interesting than that above alluded to. Don Mariano la Gasca, pupil of Cavanillez, a young botanist of great promise, has just presented me with a specimen of Lichen *(Islandicus)* which he has discovered in the mountains of Leon, where it grows plentifully.

I expected to find in it merely a weaker or stronger tinctorial matter; but I find when properly boiled it is very good to eat, is very tender, and, I think ought to be con-

* Annales de Chimie, Vol. LVII. p. 196.—February 1806.

sidered

sidered a resource for food provided by Nature perhaps in every country, which has hitherto been overlooked. I would recommend you to draw the attention of the botanists at Paris towards this plant, and provide some yourself for the entertainment of your friends; it is an excellent culinary vegetable. I think I once saw some at Vincennes or in the *Bois de Boulogne*.

Particular account of the good qualities of the Lichen.

One pound of this Lichen, dry, produced three pounds when dressed and well drained: it may be eaten with oil, butter, and no doubt in many other ways. We have already had it six times on our table, and my friends were much pleased with it. Its texture is purely membranaceous, containing neither wood nor filaments, which renders it a very agreeable food. It may be reasonably expected, that in so numerous a family, other species may be found equally nourishing, and perhaps more so. Although very elastic after being dressed, it contains not the least animal matter; for its products are similar to those of sugar, which has surprised me. A pound of this Lichen will make eight pounds of soup, which in cooling, turns to jelly, like that made from animal food. It is slightly bitter, but not more so than weak chicory water. I seasoned some with sweet and bitter almonds, lemon peel, and sugar, and it made me a very nutritious and agreeable dish. The mucilage of this plant is gelatinous, very different from gum; it appears to me to resemble that obtained from fruits. I am going to examine it in other respects, and to ascertain whether this plant affords any colouring matter for dying processes. At all events it appears that Nature cannot furnish a more excellent article of food than this vegetable.

IX.

On the Means of preserving Water in long Sea Voyages, and the application of the same Means for keeping Wines. By M. L. G.*

Proposal by Berthollet to carbonize or char the inside of casks.

MR. BERTHOLLET in the year 1803 communicated to the Class of Mathematical and Physical Science of the

* Annales de Chimie, LIX p 96.

National Institute of France, the result of an experiment on the property of charcoal to preserve water. He had four months before that time filled with water two casks, one of which had its internal surface burned. The water it contained proved fit for use, and without any bad flavour; while that in the other cask, which had not been so prepared, was so much corrupted that the smell was intolerable.

Successfully carried into effect at Sea.

The Court Gazette of Petersburgh, of May 30 last, contains an account of the success which this process was attended with in the ship of Captain Krusenstern.

He writes from Kamschatka, the 8th July 1805, to Mr. Schubert, of Petersburgh, that during his stay at Copenhagen a journal * fell into his hands, in which this process is indicated by a French chemist; that he immediately caused the internal surface of 50 or 60 casks to be burned within in a much more effectual manner than is usually done in ships of war, where the charring being only slight, the advantages are also very trifling.

by Mr. Krusenstern.

During his stay at the Brazils, Mr. Krusenstern also caused the greatest part of his casks to be burned inside; and during the whole of his passage as far as the Isle of Washington, the water in these casks was constantly found to be good. In order to maintain the cleanliness of

* This journal is probably that of M. M. Pfaff and Friedlander, which was printed at Leipzic under the title of *Die neuesten entdeckungen Fransosischer gelehrten, &c.* It contains, in the Number for May 1803, an extract of the memoir of Berthollet on this subject; the author of that article thinks he recollects that Lord Macartney had before used powdered charcoal in his provision of water for his voyage to China; but this does not take away the priority of carbonizing the inner surface of the casks.—Note of the Author.

The author proceeds to express his doubts whether charcoal was really used for this purpose in that voyage, but I have thought it needless to translate his remarks, because it is certain that Lowitz, to whom the merit of the first discoveries of the active power of charcoal in purifying and otherwise changing a great number of bodies, did very early apply it to the purifying and preserving natural waters. See three volumes of Memoirs translated from Crell's Journal, and published in London in 1793, by Baldwin. The process of clarifying muddy water by a very minute addition of alum, which is mentioned in the same voyage as practised in China, has been long known, and in common use here.—N.

of these casks, he preferred the inconvenience of having his ballastage to attend to rather than fill them up with sea water, as is usual, when they were empty, which tends to hasten the corruption of the fresh water that may be afterwards put into them. On his arrival at Japan, he burned as strongly as possible every one of his water casks, and the success of this practice was still more evident during a passage of seven weeks from thence to Kamschatka.

"Our water," says he, "was constantly pure, and as good as that from the best springs; so that we have had the honor of being the first to carry so simple and so useful a practice into effect; and the French Chemist will perhaps receive some satisfaction from hearing of our happy success."

The water at sea was as tasteless as spring water.

The preceding notice is followed by an address on the part of one of the Editors of the Annales de Chimie to Mr. Berthollet, in farther explanation of the subject. He remarks, that

The coating of charcoal acts in two manners; 1. It opposes the solution of the extractive part of the wood. 2. It prevents the putrefaction of that which may have been dissolved from such parts of the wood as might not have been originally well charred, or from which the coal may have been detached.

If the charcoal were merely to be put into the cask, or the putrefaction were corrected by means of filters containing charcoal powder, the first effect would not be obtained; and the second would even cease to be produced as soon as the property of the charcoal should be exhausted.

The process for carbonizing the inner surface of casks may also afford advantages for the preservation of wines.

The same process promises to be of value in casks for wine.

Wine, as well as water, must dissolve the extractive part of wood; and its taste, particularly when it has not one which predominates, must by that means be altered. This is the reason why casks which have already been much used are preferable to those which are new.

2. This extractive part probably favours the acid fermentation, which easily takes place in sea voyages in consequence of agitation and an elevated temperature. Hence it

it is that many kinds of wine cannot be conveyed by sea or to great distances.

Wine sufficiently clarified becomes perfect in bottles. Does not this arise from its being preserved from the extractive part of wood? and may we not conjecture that it would become still more agreeable if preserved in casks charred within, and which on that account might be substituted instead of stone ware, or good glass, besides possessing that large capacity which is favourable to the last fermentation, which renders its qualities perfect?

General view of the subject.

Spiritnous liquors likewise dissolve the extractive part of wood, and receive qualities which are in some cases valued, but in others detrimental. The charred casks would prevent this effect. In a word, the casks which have received this preparation may be used for all the purposes in which liquids are to be preserved, without being affected by the extractive part of wood, and they prevent the putrefaction to which some of them may be subject.

These views are perhaps carried too far, and may require to be supported by experience. The observations here given may serve to direct the proceeding for this purpose, which cannot but be interesting to chemists as well as others.

X.

A Chemical Examination of the Hepatic Ore of Mercury from Idria. By M. Klaproth.

Analysis of the hepatic ore of Mercury.

THE compact hepatic Mercury employed in this analysis, is of a colour which holds a middle rank between a deep cochineal red and the grey of lead; it is almost always found in compact masses. The faces of contact are brilliant. It exhibits a very slight metallic lustre in its fracture; is opake; its powder is of a deep brown red; and the scraped part shinesa little. It is tender, not brittle, and has a specific gravity of 7, 1.

The polish it takes is bad, and in this state it appears

pears of a clear liver-coloured brown, whence it has its name.

Analysis of the hepatic ore of Mercury.

A. 1000 grains of this ore, distilled with half its weight of iron filings, afforded 818 grains of pure mercury, the residue consisted of sulphuret of iron mixed with a black powder, soiling the fingers like soot.

B. a. 100 grains reduced to fine powder were heated in 500 grains of muriatic acid to ebullition.

Sulphurated hidrogen gas was disengaged. The mineral was decomposed by adding, a little at a time, 100 grains of nitric acid; a black residue of ten grains remained. This residue was burned in a porcelain capsule very carefully, in order that the sulphur only might be burned. There remained three grains of a light coaly powder, which became ignited and burned by a stronger heat, leaving one grain of reddish ashes.

b. The solution was precipitated by the muriate of barytes. The sulphate of barytes, which was obtained after having been made red hot, weighed 46,5 grains; so that there were 6,5 grains of sulphur converted into sulphuric acid by the action of the nitric acid. Estimating the quantity of sulphur contained in the sulphurated hidrogenous gas at 0,25 grains, we have 13,75 parts of sulphur in 100 of the mineral.

C. a. 1000 grains of hepatic mercury in powder were put into a retort adapted to the pneumatic apparatus; the heat was gradually raised till the residue became red hot. After the first heat had driven out the atmospheric air, sulphurated hidrogen gas issued forth, which burned with a blue flame; its volume was 24 cubic inches, without reckoning that which had been absorbed by the water put into the intermediate receiver, which was strongly impregnated with it.

b. A few globules of mercury were collected in the receiver. In the neck of the retort was a mixture of Ethiops mineral of a greasy looking humidity, some small metallic globules, and a few small needles of cinnabar. The mercury which was mechanically extracted from this mixture weighed 3,17 grains. The posterior part of the neck of the retort was alone covered with a

Analysis of the hepatic ore of Mercury.

solid sublimate of pure cinnabar which weighed 2,56 grains.

c. The residue appeared in the form of a coaly powder resembling soot, and weighed 39 grains. When burned in the open air in a roasting test, it left 16 grains of ashes; so that the carbon consumed amounted to 23 grains.

d. The earthy residue was digested with muriatic acid. Silex remained at the bottom, which after ignition weighed 6,5 grains.

e. The muriatic solution, which was of a yellow colour inclining to light green, was supersaturated with ammonia; a brown viscid precipitate fell down, and the fluid assumed a light blue tinge. The precipitate when dissolved in an hot alkaline lixivium left the oxide of iron, which was attracted by the magnet after having been ignited, and weighed 2 grains.

f. Muriate of ammonia was poured into the alkaline fluid, and threw down alumine, which after ignition weighed $5\frac{1}{2}$ grains.

g. The other ammoniacal fluid was supersaturated with muriatic acid. A bar of zinc immersed therein separated 0,20 grains of metallic copper.

On collecting the results of this analysis of the hepatic ore of Idria we find that 1000 parts consist of

Mercury................	818	
Sulphur................	137	50
Charcoal................	23	
Silex..................	6	50
Alumine................	5	50
Oxyde of Copper	2	
Copper	0	20
Water which served to form the sulphurated hidrogen gas, and other loss......	7	30
	1000	

This analysis may serve to rectify the false notions which have been adopted concerning the composition of this

this mixed mineral. By shewing that the sulphur is combined with the metal in the same proportion as in cinnabar, namely as 1 to 6 in round numbers, we are taught how little foundation there is for the opinion of those who, like Sage and Kirwan, think that a part only of the mercury is in the state of sulphurated mercury, and that the other is in the state of a simple oxide. If that were the case, the non-sulphurated part would certainly be soluble in the nitric acid. Experiment shews that this is not the case, because the acid cannot dissolve any part, even when boiling, the mineral powder remaining unchanged at the bottom of the vessel. This opinion has perhaps been taken up from observing that in sublimation a part only of the mineral rises in the state of cinnabar, while the other passes in the form of fluid mercury. But this arises from the presence of charcoal among its ingredients, which decomposes cinnabar at an elevated temperature: whether it be that the carbon takes from the mercury the minimum of oxygen necessary to the formation of cinnabar, or whether it be that the sulphur which combines at an high temperature with the carbon, and forms carbonated sulphur, is put into a state in which it cannot combine chemically with the mercury. The facts shew that it is really so; for having as a direct proof sublimed artificial cinnabar with lamp black, the greatest part of the cinnabar was decomposed in the same manner as the hepatic mercury, and the result was a mixture of Ethiops mineral and globules of metallic mercury.

Analysis of the hepatic ore of Mercury.

As an observation on the state in which mercury exists in cinnabar, I shall add that the antient opinion that it has the state of a perfect oxide cannot be maintained from the proofs which have been given by Proust, Bucholz, and others.

But does cinnabar absolutely contain no oxigen? and is the mercury in the metallic state? For my part, I think the question requires to be examined more exactly. From the appearances it seems that the mercury must exist in cinnabar at a very low degree of oxidation; which on that account has not yet been examined by observers. On this question, respecting which the present

sent limits do not permit me to say more, it must be observed, 1. that in cinnabar, whether natural or artificial, the metallic base, like all the other metals, at their lowest degree of oxidation, resists solution in the nitric acid; 2. that in the fabrication of cinnabar in the dry way, is always accompanied with an inflammation which appears to me to be an oxidation.

XI.

On the Quantity and Velocity of the Solar Motion. By WILLIAM HERSCHEL, *L.L. D. F. R. S. From the Philosophical Transactions for* 1806.

Investigation of the proper motion of the sun.

THE direction of the solar motion having been sufficiently ascertained in the first part of this paper*, we shall now resume the subject, and proceed to an inquiry about its velocity.

The proper motions, when reduced to one direction, have been called quantities, to distinguish them from the volocities required in the moving stars to produce those motions. It will be necessary to keep up the same distinction with respect to the velocity of the solar motion; for till we are better acquainted with the parallax of the earth's orbit, we can only come to a knowledge of the extent of the arch which this motion would be seen to describe in a given time, when seen from a star of the first magnitude placed at right angles to the motion. There is, however, a considerable difference between the velocity of the solar motion and that of a star; for at a given distance, when the quantity of the solar motion is known, its velocity will also be known, and every approximation towards a knowledge of the distance of a star of the first magnitude will be an approximation towards the knowledge of the real solar velocity; but with a star it will be otherwise; for though the situation of the plane in which it moves is given, the angle of the direction

* Phil. Trans. for 1805, page 231; or see our Journal, Vol. XIII. p. 59.

of

of its motion with the visual ray will still remain unknown.

Investigation of the proper motion of the sun.

As hitherto we have consulted only those proper motions which have a marked tendency to a parallactic centre, we ought now, when the question is to determine the velocity of the solar motion, to have in view the real motion of every star whose apparent motion we know; for as it would not be proper to assign a motion to the sun, either much greater or much less than any real motion which may be found to exist in some star or other, it follows that a general review of proper motions ought to be made before we can impartially fix on the solar velocity; but as trials with a number of stars would be attended with considerable inconvenience, I shall use only our former six in laying down the method that will be followed with all the rest.

Proportional Distance of the Stars.

We are now come to a point no less difficult than essential to be determined. Neither the parallactic nor real motion of a star can be ascertained till its relative distance is fixed upon. In attempting to do this it will not be satisfactory to divide the stars into a few magnitudes, and suppose *these* to represent the relative distances we require. There are not perhaps among all the stars of the heavens any two that are exactly at the same distance from us; much less can we admit that the stars which we call of the first magnitude are equally distant from the sun. And indeed, if the brightness of the stars is admitted as a criterion by which we are to arrange them, it is perfectly evident that all those of the first magnitude must differ as much in distance as they certainly do in lustre; yet imperfect as this may be, it is at present the only rule we have to go by.

The relative brightness of our six stars, may be expressed as follows: Sirius - - - Arcturus - Capella ; Lyra - - Aldebaran , Procyon.

The notations here used are those which have been explained in my first Catalogue of the relative Brightness of the Stars*; but to denominate the magnitudes of these six

* Phil. Trans. for 1796, page 189.

stars,

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stars, so that they may with some probability represent the distances at which we should place them according to their relative brightness, I must introduce a more minute subdivision than has been commonly admitted, by using fractional distinctions, and propose the following arrangement.

Table VIII.

Proportional Distances of Stars.

Sirius	1,00	Lyra	1,30
Arcturus	1,20	Aldebaran	1,40
Capella	1,25	Procyon	1,40

The interval between Sirius and Arcturus is here made very considerable; but whoever will attentively compare together the lustre of these two stars, when they are at an equal altitude, must allow that the difference in their brightness is fully sufficient to justify the above arrangement.

The order of the other four stars is partly a consequence of the distance at which Arcturus is placed, and of the comparative lustre of these stars such as it has been estimated by observations. But if it should hereafter appear that other more exact estimations ought to be substituted for them, the method I have pursued will equally stand good with such alterations. I have tried all the known, and many new ways of measuring the comparative light of the stars, and though I have not yet found one that will give a satisfactory result, it may still be possible to discover some method of mensuration preferable to the foregoing estimations, which are only the result of repeated and accurate comparisons by the eye. Whenever we are furnished with more authentic data the calculations may then be repeated with improved accuracy.

Effect of the Increase and Decrease of the Solar Motion, and Conditions to be observed in the Investigation of its Quantity.

The following table, in which the 2d, 4th, and 5th columns contain the sides of the parallactic triangle, is calculated with a view to show that an increase or decrease of

of the solar motion will have a contrary effect upon the required real motions of different stars; and as we are to regulate the solar velocity by these real motions, an attention to this circumstance will point out the stars which are to be selected for our purpose.

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Table IX.

Stars and relative Distances.	Apparent Motion.	Solar Motion.	Parallactic Motion.	Real Motion.	Velocities.
Sirius 1,00	1",11528	1,0	0",67768	+ 0",46518	465175
		1,5	1,01652	+ 0,21701	217007
		2,0	1,35536	— 0,32776	327755
Arcturus 1,20	2",08718	1,0	0,53579	+ 1,57389	1388670
		1,5	0,80368	+ 1,30478	1565735
		2,0	1,07158	+ 1,01561	1218736
Capella 1,25	0",46374	1,0	0,79593	— 0,42159	526987
		1,5	1,19390	— 0,79637	995465
		2,0	1,59186	— 1,18662	1483270
Lyra 1,30	0",32435	1,0	0,32542	— 0,47065	611839
		1,5	0,48812	— 0,59923	778995
		2,0	0,65083	— 0,74135	963750
Aldebaran 1,40	0",12341	1,0	0,65117	— 0,53208	744913
		1,5	0,97676	— 0,8 737	1200324
		2,0	1,30234	— 1,1 283	1655967
Procyon 1,40	1",23941	1,0	0,66394	+ 0,59548	833665
		1,5	0,99591	+ 0,30731	430227
		2,0	1,32788	— 0,23385	327390

The real motion of Arcturus contained in the 5th column compared with that of Aldebaran, shows that when the solar motion is increased from 1,0 to 1,5 and to 2",0 the real motion of Arcturus will be gradually diminished from 1,57 to 1,30 and to 1",02, while that of Aldebaran undergoes a contrary change from 0,53 to 0,86 and to 1",18. We may also notice that Capella and Aldebaran,

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Aldebaran, which have a negative sign prefixed to their real motions when the solar motion is 1″,0 are affected differently from Arcturus, Sirius, and Procyon, which have a positive sign; and that even the motions of the two last become negative when the solar motion is increased beyond a certain point. It may be easily understood that the motion of Arcturus itself would become negative were we to increase the solar motion till the parallactic motion of this star should exceed its apparent motion.

From these considerations it appears, that a certain equallization, or approach to equality may be obtained between the motions of the stars, or between that of the sun and any one of them selected for the purpose; for instance, the motions of Arcturus and Aldebaran being contrary to each other, may be made perfectly equal by supposing the sun's annual motion to be 1″,85925. For then we shall have the real annual motion of Arcturus towards the parallactic centre 1″,091, and that of Aldebaran towards the opposite part of the heavens, in which the solar apex is placed, will be 1″,091 likewise; the first in a direction 55° 29′ 39″ south-preceding, the latter 88° 16′ 31″ north-following their respective parallels; and a composition of these motions with the parallactic ones arising from the given solar motion, will produce the apparent motions of these stars which have been established by observation. But since Arcturus, by the hypothesis which has been adopted in Table VIII. is a nearer star than Aldebaran, the velocities of the real motions, describing these equal arches will be 1309109 in the former and 1527780 in the latter. And it is not the arches but these velocities that must be equalized. Therefore, in order to have this required equality, let the solar motion be 1″,718865 then will a velocity of 1399478 in Arcturus, and 1399842 in Aldebaran, which are sufficiently equal, occasion such angular real motions in the two stars as will bring them, when compounded with their parallactic motions, to the apparent places in which we find them by observation.

Before we proceed, it will be proper to obviate a remark that may be made against this way of equalization or

or approach to equality. We have said that the calculated velocities are such as would be true if the stars were at the assumed distances, and if their real motions were performed in lines at right angles to the visual ray; to which it may here be objected that the last of these assumptions is so far from having any proof in its favour, that even the highest probability is against it. We may admit the truth of what the objection states, without apprehending that any error could arise on that account, if the solar motion were determined by this method. For if the stars do not move at right angles to the visual ray, their real velocity will exceed the calculated one; so that in the first place we should certainly have the minimum of their velocities: and if we were obliged, for want of data, to leave the other limit of the motion unascertained, it must be allowed to be a considerable point gained if we could shew what is likely to be the least velocity of the solar motion; but a more satisfactory defence of the method is, that if we were to assume a mean of all the angular deviations from the perpendicular to the visual ray that may take place in the directions of the real motions of the stars, the only position we could fix upon as a mean would be an inclination of 45 degrees. For in this case the chance of a greater or smaller deviation would be equal; and when a number of stars are taken, the deviations either way might then be supposed to compensate each other; but what is chiefly to our purpose, not only the angle of 45 degrees, but also any other, that might be fixed upon as a proper one to represent the mean quantity of sideral motions, would lead exactly to the same result of the solar velocity to be investigated. For if the velocities of any two stars were equalized, when their motions are supposed to be perpendicular to the visual ray, they would be as much so when they make any other given angle with it; and it is the equalization or approach to equality and not the quantity of the velocities that is the spirit of this method. I have only to add, that an equalization of the solar motion with that of any star selected for the purpose may be had by a direct method of calculation, and will therefore be of great use in settling the rate of the motion to be determined.

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It must be evident from what has been said, that a certain mean rate, or middle rank, should be assigned to the motion of the sun, unless very sufficient reasons should induce us to depart from this condition. To obtain this end must consequently be our principal aim; and if we can at the same time bring the sidereal motions to a greater equality among each other, it will certainly be a very proper secondary consideration.

There are two ways of taking a mean of the sidereal motions, one of them may be called the rate and the other the rank. For instance, a number equal to the mean rate of the six numbers, 2, 6, 13, 15, 17, 19, would be 12; but one that should hold a middle rank between the three highest and three lowest of the six would be 14. In assigning the rate of the solar motion it appears to be most eligible that it should hold a middle rank among the sidereal velocities. We shall however find that nearly the same result will be obtained from either of the methods.

With respect to our second consideration, we may see that it also admits of a certain modification by the choice of the solar motion; for in Table IX. when this motion is 1",5 the velocity of Arcturus 1565735, will exceed that of Sirius, 217007, more than seven times; whereas a solar motion of 1" will give us the proportional velocities of these stars as 188867 to 465174; and the former will then exceed the latter only four times.

Calculations for drawing Figures that will represent the observed Motions of the Stars.

The necessary calculations for investigating the solar motion are of considerable extent, and may be divided into two classes, the first of which will remain unaltered whatsoever be the solar motion under examination, while the other must be adjusted to every change that may be required.

The direction of the sun remaining as it has been settled in the first part of this Paper, the permanent computation of each star will contain the annual quantity of the observed or apparent motion, its direction with the parallel of the star, its direction with the parallactic motion, and

and its velocity. The changeable part will consist of the angular quantity of the real motion, the parallactic direction of this motion, and its velocity.

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Before we can make a calculation of the required velocities, we must fix upon the probable relative distance of the rest of the stars, in the same manner as we have done with the first six. In this I have thought it advisable to distinguish the stars that, from their lustre, may be called principal, and have limited their extent to the brightest of the second magnitude, on account of the uncertainty which still remains about their progressive distances. For though it appears reasonable to allow that the bright stars of the second magnitude may be twice as far from us as those of the first, it will admit of some doubt whether this rule ought to be strictly followed up to the 3d, 4th, 5th, and 6th magnitude; especially when it is not easy to ascertain the boundaries which should limit the magnitudes of very small stars.

The number of these principal stars is 24. The remaining 12 are also arranged by admitting that their magnitudes express their relative distances; and notwithstanding the doubtfulness we have noticed, their testimony with respect to the proper quantity of a solar motion, though it should be received with some diffidence, must not be neglected; some considerable alteration in their supposed distances, however, would have but little effect upon the conclusions intended to be drawn from their velocities.

The following Table contains the result of the calculations that relate to the permanent quantities. In the first and second columns, we have the names of the stars, and their assigned relative distances. The third gives the apparent angular motions, and the fourth their direction. The fifth contains the direction of the same motions, with respect to the parallactic motions arising from the given solar direction; and the sixth gives the velocity of the stars which produce the quantity of the apparent motions.

Table X.

Names of the Stars.	Proportional Distances.	Apparent Motions.	Direction with the Parallel.	Direction with the parallactic Motion.	Velocity of the Stars.
Sirius	1,00	1″,11528	68.19.10,7 sp	10.21.41,3 sf	1115281
Arcturus: ..	1,20	2,08718	55.29.12,0 sp	0. 0. 3 sp	2501621
Capella.	1,25	0,46374	71.35.22,4 sf	24.40.21 sf	579668
Lyra.	1,30	0,32135	56.20.57,3 nf	92.49.30 nf	421657
Rigel	1,35	0,16273	79.29.33,9 np	159.28. 1 np	219684
αOrionis....	1,35	0,13038	85.38.14,6 nf	169.18.58 np	176010
Procyon....	1,40	1,23941	50. 2.24,5 sp	9.40.46 sp	1735172
Aldebaran ..	1,40	0,12341	76.29.37,3 sf	13.41.48 sf	172778
Pollux	1,42	0,65037	0. 0. 0 *prec.*	61.30.34 sp	923523
Spica.	1,44	0,19102	84. 5. 1,8 np	144.13.16 np	275065
Antares	1,46	0,26000	90. 0. 0 *north*	178.57.44 np	379600
Altair......	1,47	0,71912	48.40.12,0 nf	103.17.29 nf	1057105
Regulus	1,48	0,22886	20.27.37,5 np	70. 9.20 sp	338711
βLeonis	1,50	0,55324	7.16. 8,4 sp	40.34.31 sp	829856
βTauri.	1,50	0,10039	84.58.27,1 sf	13.17.11 sf	150579
Fomalhaut..	1,50	0,30698	11.16.16,3 nf	16.47. 5 sf	460469
αCygni.	1,60	0,06440	27.45.56,3 np	177.31.39 np	103036
Castor	2,00	0,13294	17.30.40,6 sp	45.25.43 sp	265869
αOphiuchi ..	2,00	0,07698	40.30.24,8 sf	33.29.28 sf	153955
αCoronæ...	2,00	0,23279	7.21.15,4 sf	105 0.43 nf	465587
αAquarii ...	2,00	0,20615	67.10.17,1 np	162.43.46 nf	412295
αAndromedæ	2,00	0,09268	40.20.48,2 sf	12.55.11 sf	185360
αSerpentis ..	2,00	0,21913	60. 7.12,5 nf	161.34. 4 nf	438257
αPegasi	2,00	0,18917	72. 5.16,0 np	157.45.25 nf	378338
αHydræ	2,30	0,16598	57.30.24,8 np	107. 6.24 np	381763
α^2 Libræ	2,40	0,18376	54.42.52,9 np	127. 3. 7 np	441022
γPegasi.	2.50	0,17355	59:48. 7,9 np	174. 5.15 nf	433880
αArietis	2,50	0,11587	37. 9.15,9 sf	29.32.47 sf	289685
αCeti	2,80	0,14406	33.44. 2,9 np	141.18.55 np	403356
αHerculis. ..	3,00	0,23000	90. 0. 0 *north*	168.23.41 nf	690000
βVirginis ..	3,00	0,77706	17.59.25,5 sf	111.11.44 nf	2331169
γAquilæ....	3,00	0,19320	55.54.41,7 np	178.25.20 nf	579589
α^2 Capricorni	3,50	0,26452	79.23.35,3 nf	136.21.18 nf	925819
βAquilæ....	4.00	0,35127	85. 7.37,0 sp	39.49.15 sp	1405079
α' Capricorni	4,20	0,28000	90. 0. 0 *north*	146.59.44 nf	1176000
α' Libræ	6,00	0,20898	59.27.58,4 np	131.46. 7 np	1253875

The contents of this Table will enable us to examine the motions of the stars in different points of view. For instance, by the apparent motions in the third column, and their directions in the fourth, a figure may be drawn which will represent the actual state of the heavens, with respect to those annual changes in the situations of our 36 stars, which in astronomical tables are called their proper motions.

Investigation of the proper motion of the sun.

Fig. 1, Plate VI. gives us these motions brought into one view, so that by supposing successively every one of the stars to be represented by the central point of the figure, we may see the angular quantity and direction of the several annual proper motions represented by the line which is drawn from the centre to each star. By this means we have the comparative arrangement and quantity of these movements with respect to their directions.

Fig. 3 represents the same motions, but instead of being drawn so as to show their directions with regard to the several meridians and parallels of the stars, they are laid down by the angles contained in the fifth column; and will therefore indicate their arrangement with respect to a line drawn from the solar apex toward the parallactic centre. These directions will remain the same, whatever may be the velocity of the solar motion upon which we shall ultimately fix, provided no change be made in the situation of the apex toward which the sun has been admitted to move.

In these two figures, the lines drawn from the centre give us only the angular changes of the places that have been either observed or calculated, and not the velocities which are required in the stars to produce them. It will therefore be necessary to represent the velocities by two other figures, in which the same directions are preserved, but where the extent of each line is made proportional to the distance of the stars in the second column.

Fig. 2 is drawn according to this plan; the angles of the directions remain as in the fourth column, but the lines are lengthened so as to give us the velocities contained in the sixth.

In Fig. 4, the angles of the 3d figure are preserved, but the lines are again lengthened as in Fig. 2.

N. B. These

N. B. These two last figures would have been of an inconvenient size if they had been drawn on the same scale with the two foregoing ones, for which reason, in comparing the 2d and 4th with the 1st and 3d, it must be remembered that the former are reduced to one half of the dimensions of the latter.

[The Conclusion in our next.]

XII.

Discovery of a New Vegetable Principle in Asparagus. (Asparagus Sativus. Linn.). By Messrs. VAUQUELIN *and* ROBIQUET*.

New vegetable principle in asparagus.

BY examining more attentively than was formerly done the products of vegetation, modern chemists have distinguished a great number of substances unknown to the antients; but it is a long time, I apprehend, since any immediate principle has been discovered which is so singular and interesting as that of which we are about to speak.

During the last summer, Mr. Robiquet, a young chemist, who unites solidity of reasoning to a great skill in experiments, subjected upon the invitation of Mr. Parmentier, the juice of asparagus to chemical analysis, of which the interesting results are to be found in our Annals.

Two kinds of crystals formed in the juice of asparagus.

Having set aside in my laboratory, during a journey which he made, a certain quantity of the juice of asparagus, concentrated by evaporation, I observed a considerable number of crystals, among which two appeared to me to belong to new substances; and as their form, transparency and taste were different, it was easy for me to separate them.

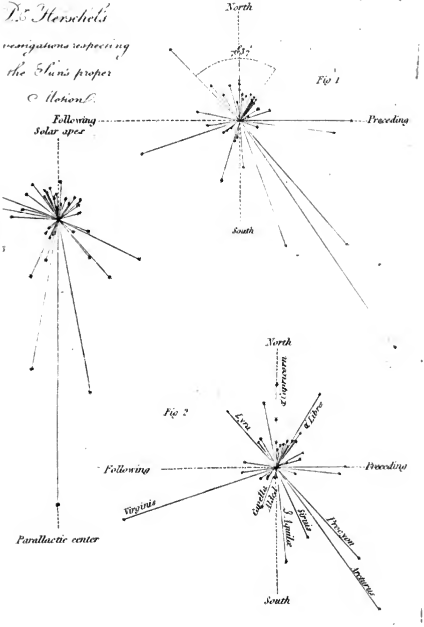
Description of one kind

One of these kinds was perfectly white and transparent after having been several times crystallized.—Its taste is cool and slightly nauseous, so as to occasion a

* Annales de Chimie, Jan. 1806.

secretion

Dr. Herschel's investigations respecting the Sun's proper Motion.



secretion of the saliva; it is hard, brittle, and of a regular form.

The other kind which is also white, is not so transparent nor hard, neither has it the same form; on the contrary, it has little consistence, is crystallized in the shape of fine needles, and its taste is perceptibly saccharine, resembling that of manna. *and of the other.*

Mr. Robiquet in making the analysis we have alluded to, had noticed the first of these substances, but he took it for an ammoniacal salt, because in the very small quantity of imperfectly purified crystals he could then obtain, it retained between its plates. According to all appearance, some traces of salt with base of ammonia, with which the juice of asparagus abounds, and which misled him. Since that time we have in conjunction submitted this substance to new experiments, the principal of which follow. The form which it effects in its crystallization, according to M. Hauy, to whom we sent a certain quantity, is derived from a right rhomboidal prism, of which the great angle of the base is about 130 degrees. The borders of this base, and the two angles, situated at the extremity of its greater diagonal, are replaced by facets. *Figure of the first kind of crystals.*

This substance is moderately soluble in water, and its solution gives no signs of acid or alcali. The infusion of nut galls, the acetate of lead, the oxalate of ammonia, the muriate of barytes, and the hydro-sulphuret of potash, produced no change in its solution. Alcohol does not dissolve it. *They are soluble in water, and neither acid nor alkaline.*

As these experiments indicate that the substance in question is not a salt with an earthy base, we triturated a certain quantity with caustic potash and a little water, in order to see whether ammonia would be disengaged; but no traces were exhibited. The potash appeared to us to render it more soluble in water. *They are not earthy nor ammoniacal.*

As we saw that it contained neither earth nor ammonia, we directed our enquiries to ascertain the existence of the alcalis, and for that purpose we burned a somewhat considerable quantity in a crucible of platina. At first it swelled up and emitted penetrating vapours which affected the eyes and the nose like the smoke of wood. It afforded a large portion of charcoal, which had no taste and left *—nor neutral.*

left nothing after its incineration but an almost imperceptible trace of earth, which no doubt was casually present.

Towards the end of the decomposition of this substance, the odour which was disengaged was somewhat similar to that of animal matter, and likewise inclining to that of ammonia.

Action of nitric acid.

The nitric acid decomposes this substance, nitrous gas being disengaged while the fluid assumes a yellow colour and a bitter taste, like animal substances. When the action of the nitric acid is completed, lime disengages abundance of ammonia from the liquid.

This alkali is therefore formed in the operation we have described, since the substance from asparagus did not afford perceptible signs before.

General conclusions.

This substance is not an acid since it does not redden the tincture of tarnsole, and has not the taste which all these substances have in a more or less eminent degree.

It is not a neutral salt because it contains neither earth nor alkali; but as it affords by means of fire the same products as vegetables, we are obliged to consider it as an immediate principle of asparagus.

It is probable that like them it is composed of hidrogen oxigen, and carbon, in particular proportions; it is no less probable that it has likewise a small quantity of azote; this at least seems to be indicated by the smell, which is disengaged by heat, and the ammonia which it forms with the nitric acid.

The authors intend to pursue their experiments.

Though we have obtained a considerable quantity of this substance, we have not been able to submit it to a greater number of experiments, because most part of it was scattered in our laboratory, and there only remained the little portion which we gave to Mr. Hauy to determine its form. We have nevertheless thought it proper to communicate these facts to the Institute, in order to fix the date of its discovery, and it is our intention to proceed in our examination on the return of the asparagus season. We shall also endeavour to ascertain whether this singular matter do not exist in other vegetables.

The second kind of crystals was probably manna.

With regard to the saccharine matter which we also found in the juice of asparagus, we had not a sufficient quantity

quantity to ascertain what species of sugar it might most nearly resemble; we take it to be manna.

We may therefore consider it as decided that besides the principles discovered in the juice of asparagus by Mr. Robiquet, there exists in it a principle which is crystallizable like the salts, but is neither acid nor neutral, and of which the solution in water is not affected by any of the reagents usually employed to ascertain the presence and nature of the salts dissolved in water; and also another principle which appears to resemble manna. Conclusion.

XIII.

A Chemical Examination of Native Cinnabar. By M. KLAPROTH.

I. *The Cinnabar of Japan.*

THE cinnabar of Japan is brought to Europe in the form of single grains, more or less large and crystalline. Its colour is of a deep cochineal red, approaching the grey colour of steel in the places which are not damaged; in others it is of a scarlet red, inclining to a brick-dust colour. The grains are fragments of flattened hexahedral prisms; externally smooth and of a metallic lustre; internally very bright and of a semi-metallic lustre. Their fracture crosswise is conchoidal, but longitudinally, it is obscurely lamellated. This mineral is tender, its scrapings of a scarlet red, and its specific gravity=7.710. Its fragments sometimes include specks of pyrites, and in other instances they adhere to a quartzose gangue. In order to distribute these heterogeneous parts uniformly through the mass of grains subjected to analysis, they were mixed and pounded together. External characters of the cinnabar of Japan.

A. One thousand grains of this powder were sublimed in a small glass retort with a receiver adapted thereto, and filled with water. The water of the receiver acquired a turbid yellowish appearance from the particles of sulphur, which were volatilized. It had a faint smell of sulphurated hidrogen, and a slight taste of sulphureous acid. The matter remaining in the retort weighed 38 grains. It was digested Impurities separated from the cinnabar by heat.

digested with muriatic acid; the iron from the pyritous particles was dissolved and the quartzose gangue remained.

Solution of the metallic part.

B. One hundred and four grains of the mineral, which from the preceding experiment contained 100 grains of pure cinnabar, were reduced to an extremely fine powder, and put into 500 grains of muriatic acid (sp. grav. 1,125) and heated: sulphurated hidrogen gas was disengaged. Into the solution was poured drop by drop, 100 grains of nitric acid (sp. grav. 1,235). Every time the acid was added, there was an immediate effervescence. In this manner the process was carried on till the decomposition of the cinnabar, and the complete solution of the metallic parts were effected.

Combustion of the sulphur.

b. The sulphur that remained was of a greylsh yellow colour and in some degree viscid: it weighed 11,8 grains. It was burned in a roasting test and left a blackish residue of 1,5 grains, so that the contents of pure sulphur were 10,3.

c. The lively action of the nitric acid upon cinnabar, gives reason to believe that part of the sulphur was converted into sulphuric acid, by the oxigen of the decomposed nitric acid.

Deduction of the quantity of sulphur which had been acidified, and thence the proportion of mercury and sulphur in the cinnabar.

In order to ascertain the quantity of sulphur which had undergone this change, the solution of the metallic part of the cinnabar (which was of a yellow colour, on account of iron) was taken and decomposed by means of a solution of muriate of barytes. The sulphate of barytes which fell down, after having been ignited, weighed 30 grains; which answers to 4,2 grains of sulphur. A small quantity likewise escaped which has contributed to the formation of sulphurated hidrogen gas; but as this quantity did not exceed one fourth of a grain, we may conclude that 100 parts of pure cinnabar contain 14,75 of sulphur.

Analysis in the dry way afforded the same result.

C. 1040 grains of the mineral, containing, according to the essay A, 1000 grains of pure cinnabar, were mixed with half their weight of iron filings, and distilled in a suitable apparatus: the mercury thus obtained, being carefully collected weighed 845 grains.

From

From these essays we may conclude that 100 parts of Japan cinnabar, exclusive of its foreign parts, contain

Mercury . . .	84,50
Sulphur . . .	14,75
	99,25

II. *Cinnabar of Neumaerktel in Carniola.*

External characters of the cinnabar of Carniola.

Among the cinnabar mines of Europe, that of *Terhitz* on the mountain of *Loibl* near Neumaerktel, in Carniola, is particularly distinguished by the beauty of the specimen it affords.

The colour of this mineral is of a lively cochineal red. It is found in masses of considerable size, in a compact calcareous stone, of a blackish grey, and crossed by veins of white calcareous spar. The faces of contact of the ore against its gangue are brilliant, with a metallic aspect; the cross fracture is of little brilliancy, of a shining greasy aspect. It is obscurely luminated and irregular in other directions. The fragments are of an indeterminate form with obtuse edges. The masses are composed of thin separate laminæ, striated on the faces of separation. This mineral is translucid; its scrapings or powder of a very lively scarlet; it is very tender, and weighs 8,16.

Analysis as before in the dry way.

A. 100 grains of this cinnabar were reduced to very fine powder, and then boiled in 500 grains of muriatic acid. Sulphurated hidrogen gas was separated, and 100 grains of nitric acid was gradually added. The metallic part having been entirely dissolved, there remained 10,20 grains of sulphur of a clear yellow, which being burned on a test, left no residue. The muriate of barytes precipitated 27 grains of sulphate of barytes, containing 3,80 grains of sulphur. Admitting three fourths of a grain of this substance to have existed in the sulphurated hidrogen gas, it will follow that 1425 parts of sulphur were contained in 100 of the cinnabar.

The same by heat.

B. Five hundred grains of cinnabar were distilled with half its weight of iron filings. The mercury obtained from this operation, carefully collected, weighed 425

Kk 2 grains,

grains, whence it follows that 100 parts of cinnabar, contain by analysis

Mercury	. .	85,0
Sulphur	. .	14,25
		99,25

XIV.

Notice of some Experiments made by the Galvanic Society at Paris.*

Pile without any moisture.

I. M. MARECHAUX, of Wesel, correspondent of the Galvanic Society, announced to them that he had determined that water, whether pure or mixed with an acid, or charged with any salt, is not indispensably required for producing the effects of galvanism. He added, that some time ago he had constructed columns of zinc and brass with the interposition of discs of card, *not moistened*, which were very useful. The Galvanic Society was of course desirous of verifying a fact of this nature, and determined to repeat the experiments of M. Marechaux, as described in his letter.

—verified by experiment.

Discs of zinc, which had been before used, were cleaned and restored to their usual polish. Similar pieces were made out of new *brass*. A vertical column of 49 pieces of discs was formed by the interposition of pieces of card, *not moistened*, standing upon a plate of brass, of greater dimensions, having three holes near its edges, through which, cords of silk were passed in order to support the whole apparatus. These cords were tied together at top, and the whole column suspended by them. This pile which M. Marechaux distinguishes by the name of *Colonne pendule*, was put into communication with the electric micrometer of M. Marechaux, simplified by M. Veau de Launay† and it manifested an intensity of 360 degrees‡, which

It was weak.

* Annales de Chimie, Jan. 1806.

† See Journ. de Phys. Messidor, an. XIV.—See also our Journal, XIV. p. 350.—N.

‡ By intensity we denote the measure of the distance, at which a leaf

which was ascertained to be the effect of galvanic action, and not from the electricity of the atmosphere.

The same repeated.

This first experiment was repeated and varied in different ways. Blotting paper was substituted instead of the pieces of card to the number of four for each, and there was no effect produced. Discs of card, dried in the oven were used, and the mean term of attraction in several experiments was 372 degrees. With the same pieces and twenty-five pair of discs only, the attraction was 160. The experiment was afterwards made with a column, having the same number of pairs of metallic discs but without the interposition of any pieces of card. In these circumstances no effect was produced.

Question how far they may prove useful.

These first results would have been sufficient for the Society to confirm the fact announced by M. Marechaux, and which was intended to be verified; but this galvanic action of the pendulous column was not proved, but by the help of an instrument of very great sensibility, and with regard to quantities scarcely to be estimated. It remains therefore for the Society to ascertain the advantage which it is possible to derive for the progress of galvanism, by means of a discovery so important, by employing more powerful modes of action, and by comparing them with the effects obtained from piles excited by humidity or by saline solutions. The class of the Society which is employed on physical researches, has been charged to direct its investigations.

Pile of vegetable matter only.

II. A notice appeared in the Moniteur of the 22nd of Brumaire last, that Dr. Joseph Baronio of Milan, had published a description of a galvanic pile, formed of vegetable matters only, with an invitation to philosophers to repeat and vary his experiments, flattering himself that they would serve to extend the application of the theory of galvanism to the whole of vegetable life. The Galvanic Society was called upon to answer this observation of Dr. Baronio*.

a leaf of gold, suspended to a vertical stem of brass, is attracted towards another stem of the same metal, terminating in a ball, when these two stems are in communication with the two poles of the pile. Each degree of this measure answers to the eighteen thousandth of an inch.

* Annales de Chimie, Jan. 1806.

A pile

The experiment repeated.

A pile was accordingly formed by them in his manner: sixty equal discs of walnut-tree were made, two inches in diameter, having a raised edge of one eighth of an inch high. These pieces were boiled in vinegar and with these and round pieces of raw beet root and of a thick raddish, (raphanus sativus of Linnæus,) a pile was constructed of sixty couple of pieces of beet root and raddish, separated by discs of wood, on the upper extremity of each of which was poured by means of the border, a solution of the acidulous tartrite of potash in vinegar. Lastly, at the lower extremity of the pile was placed a leaf of cochlearia, and at the upper extremity a double-band of blotting paper, steeped in vinegar. Every thing being thus disposed agreeably to the full description inserted in the Moniteur; frogs properly prepared for the action of this pile, were placed with the leaf of cochlearia in contact with their spinal marrow, and the band of paper with their muscles. Three frogs being thus successively and repeatedly presented, shewed not the least motion, though they were sufficiently sensible to be strongly agitated when being supported on a knife to bring them near the conductor of the pile, they were in contact with the blade or silver mounting of the handle. After having made every probable experiment with these frogs without success, the pile was brought into communication with the electomicrometer, upon which also it produced no effect. The same instrument was then presented to a pendulous pile, constructed after the manner of M. Marechaux, composed of 60 pair of new discs of copper and of zinc, with the interposition of pieces of card, not moistened. The intensity was about 180 degrees. At the same moment the frogs which had been presented to the vegetable pile were put into communication with this last, and they gave no indication of sensibility.

It did not succeed.

The electromicrometer more delicate than frogs.

The Galvanic Society did not therefore obtain in the experiments indicated by Dr. Baronio, the results which he announced; but they have served to show that the electro-micrometer made use of, is still more sensible than frogs, to shew the smallest effects of galvanism.

XV.

Observations on the Congelation of Water. By M. Dispan, *Professor of Chemistry at Toulouse.*

About the close of the winter of the year XI. we had at Toulouse, after several days of a temperature remarkably mild, a return of cold very sudden and strong; the canal was frozen in one or two nights, and there was skating, a spectacle very uncommon in this country. The ice remained for eight hours without thawing; but notwithstanding this, the water under the bridges was never frozen, not even slightly. This singularity was noticed with surprize by every one, and I was for a long time at a loss to discover the cause. I think I now understand it.

Remarkable fact of congelation in standing water, which did not freeze under a bridge.

The earliest and the latter frosts are called white frosts, and their cause is well understood. The white appearance is formed by the dew, which crystallizes as it falls. The hard frosts in the depth of winter are, on the contrary, called black frosts, and this expression is equally applicable to the appearance of the ground in that circumstance. For this effect it is requisite that the cold should have previously deprived the atmosphere of the moisture it contained. Nothing is precipitated; but the water upon the ground or soaked into it becomes solid.

Explanation. Black and white frosts.

In fact, when a cold night suddenly follows a succession of warm days, as happens at Toulouse at the times I mention, an abundant hoar frost succeeds. The still waters receive such a quantity, that their caloric, already in part absorbed at the surface by the coldness of the air, can no longer keep up the fluid state. The hoar frost, or precipitated ice, then forms a pellicle at the surface of the water, and by its contact determines the congelation from one part to the other to a certain thickness.

Their cause.

This is not the case with running waters. These by their continual motion prevent the hoar frost from forming a coating to the surface. The frozen particles as they fall from the atmosphere are immersed and mixed with the stream; and when the coldness of the atmosphere itself

Manner in which running water congeals.

itself determines the crystallization, the same thing happens with regard to the spicular crystals thus beginning to be formed. This is the reason why rivers always begin to freeze near their marshy sides, and at places where the current is the least rapid.

— And standing water, which explains the phenomena.

But to return to the stagnant waters. However abundant may be the deposition of hoar frost, the water beneath a bridge will receive no part of it. The surface of the water has therefore this cause of refrigeration less than at the other parts. Its caloric is not taken away but by the mere contact of the air. This condition would be sufficient to render its congelation much more slow; but its fluidity is not less preserved by its surface being defended from the predominating action of the hoar frost, which would follow if that obstacle were not interposed.

Other facts of the same kind.

These facts enabled me to explain certain experiments urged by a philosophical gentleman at Paris a few years ago, to support his opinion on the existence of a material principle under the name of *frigoric*. The author of these experiments assured me, that in a frosty night the *frigoric* fell perpendicularly from the atmosphere upon the surface of the earth; and he offered the following proof. If plates filled with water be exposed to the open air at night, and it be cold enough, the water will freeze: but if one of these vessels be covered with a pane of glass, or any other body, that water will not freeze, even though the covering body do not rest immediately upon the plate. It is sufficient, continued the author, that the fall of the *frigoric* be interrupted, no matter whether from an higher or lower distance; and to complete his demonstration, he added the following experiment, which at first aspect seems very cogent, and is certainly very interesting. It is as follows: place in the evening, at a certain distance above a plate filled with water, a funnel, of which the diameter shall be less than that of the plate, you will find the next day a ring of ice formed round the circumference; but all the water situated perpendicularly beneath the funnel will remain fluid.

Explanation.

I have not repeated this last experiment; but every thing leads me to conclude that it would succeed in favorable

able circumstances; that is to say, when the air holding a certain quantity of water in solution, shall be forced to deposit it all at once in the solid form. We see consequently that in this case, without having recourse to the existence of a frigorific principle, the hoarfrost falling on the sides of the funnel will be guided toward the edges of the plate, where a ring of ice will be formed before the middle shall become congealed.

XVI.

Practical Rules for reducing the apparent Distance of the Moon from the Sun or a fixed Star to the true Distance, for the Purpose of ascertaining the Longitude of the Place of Observation. By a Correspondent.

AT some former periods of my life I was not unfrequently in the habit of amusing myself with practical astronomy, and, amongst other departments of it, with what are usually called the common lunar observations. In the course of these it was impossible to avoid remarking that none of the rules given at the end of the "Requisite Tables" for reducing the apparent to the true distances are by any means so short, or so easy to be remembered, as might be wished; and that it would be highly desirable to diminish the labour of this process. I was, indeed, previous to the investigation of the methods hereafter described, usually accustomed to prefer the direct solution of the two triangles, in the former of which the apparent co-altitudes and observed distance are given, and the vertical angle required, and in the other of which the true co-altitudes and azimuthal angle are given, and the base or true distance required. The following methods of performing this reduction, which I then hit upon, appearing however to me to be somewhat more eligible than any others which I have seen, I shall venture to communicate them to the public through the medium of the Philosophical Journal. They are all founded on the two following well-known analogies, viz. That the rectangle of the sines of the sides containing the vertical angle

Origin of the Investigation.

Former rules prolix and burthensome to the memory.

Analogies on which the following rules are founded.

angle : the square of radius : : the rectangle of the sines of the differences between the half-sum of the three sides, and each of those first-mentioned sides : the square of the sine of half the contained angle; and : : the difference between the versed sines, (or the sum or difference of the co-sines,) of the base and of the difference of the sides : the versed sine, (or sum or difference of radius and co-sine,) of the same angle.

RULE I*.

First rule.

1. If the apparent distance be greater than 90°, take the sum, otherwise take the difference, of its natural co-sine and the natural co-sine of the difference of the apparent altitudes, and call it A.

2. Add together the arithmetical complements of the logarithmic co-sines of the observed altitudes, the logarithmic co-sines of the true altitudes, and the logarithm of A; reject 20 from the index, and find the correspondent natural number, which call B.

3. The difference between this number and the natural co-sine of the difference of the true altitudes is the natural co-sine of the observed distance, which will be greater or less than 90° accordingly as B is greater or less than the last-mentioned co-sine.

RULE II.

Second rule, without the use of natural numbers.

1. Add together the two apparent co-altitudes and the apparent distance, and take the difference between their half-sum and each of the apparent co-altitudes separately.

2. Add together the arithmetical complements of the logarithmic co-sines of the two apparent altitudes, the logarithmic sines of the two before-mentioned differences, and the logarithmic co-sines of the true altitudes, and halve the sum.

* Since writing the above I find that there is a method perfectly analogous to this in Mr. Thos. Keith's Trigonometry, published in , and differing only in his using secants in one part of the process instead of co-sines. As I think it, however, a very useful mode of reduction, I have not struck it out, but shall content myself with thus resigning to that gentleman the merit of the first publication of it. Q.

3. Subtract

3. Subtract from this half-sum the logarithmic sine of half the difference of the true altitudes, and the remainder will be a logarithmic tangent.

4. Find the correspondent logarithmic sine, subtract it from the before-mentioned half-sum, and the remainder will be the logarithmic sine of half the true distance.

RULE III.

Third rule, also without the use of natural numbers.

1. Add together the arithmetical complement of the logarithmic sine of half the apparent distance and the logarithmic sine of half the difference of the apparent altitudes, and their sum will be the logarithmic co-sine of an arc, which call A.

2. Add together half the sum of the logarithmic co-sines of the true altitudes, the logarithmic sine of half the difference of the apparent altitudes, and the logarithmic tangent of A.

3. Add together also half the sum of the logarithmic co-sines of the apparent altitudes and the logarithmic sine of half the difference of the true altitudes.

4. The difference between these sums is a logarithmic tangent of an arc, which call B.

5. To the logarithmic sine of half the difference of the true altitudes add the arithmetical complement of the logarithmic co-sine of B, and their sum will be the logarithmic sine of half the true distance.

RULE IV.

Fourth rule, analogous to the last.

1. Add together the arithmetical complement of the logarithmic sine of half the apparent distance and the logarithmic sine of half the difference of the apparent altitudes, and their sum will be the logarithmic co-sine of an arc, which call A.

2. Find the logarithmic sine of A; subtract from it the before-mentioned arithmetical complement, and double the remainder.

3. Add to this doubled remainder the arithmetical complements of the logarithmic co-sines of the apparent altitudes, and the logarithmic co-sines of the true altitudes, and halve the sum.

4. From this half-sum subtract the logarithmic sine of

half the difference of the true altitudes, and the remainder will be a logarithmic tangent.

5. Find the correspondent logarithmic sine; subtract it from the before-mentioned half-sum, and the remainder will be the logarithmic sine of half the true distance.

We will work one of the cases given in the "Requisite Tables" by each of these rules.

EXAMPLE.

Example;— Let the apparent distance of the moon from a star be 89°. 58′. 6″. the apparent altitude of the star 5°. 6′. that of the moon 84°. 46′. and her horizontal parallax 61′. 18″.; what is their true distance?

In this case the correction for the moon's parallax and refraction taken from Tab. VIII. Requisite Tables, is + 5′. 30″.; and that for the star's refraction from Table I.—9′. 44″.; so that their true altitudes are 84°. 51′. 30″. and 4°. 56′. 16″.

Then, by the First Rule.

— worked by the first rule;

Nat. cos. 79°. 40′.	.1793746
Nat. cos. 89°. 58′. 6″	.0005527
A..... =	.1788219
Ar. comp. log. cos. 84°. 46′.	1.0399483
5°. 6′.	0.0017228
Log. cos. 84°. 51′. 30″.	8.9523977
4°. 56′. 16″.	9.9983855
Log. A.	$\bar{1}$.2524208
Log. B.	$\bar{1}$.2448751
Nat. cos. 79°. 55′. 14″.	.1750135
B. nat.	.1757418
Diff.	.0007283

= nat. cos. 90°. 2′. 30″. 3. the true distance.

Or, by the Second Rule. — by the second rule;

As. comp. log. cos. 84°. 46′.	1.0399483
5°. 6′.	0.0017228
Log. sin. 5°. 9′. 3″...........	8.9531696
84°. 49′. 3″...........	9.9982210
Log. cos. 84°. 51′. 30″...........	8.9523977
4°. 56′. 16″...........	9.9983852
	2)38.9438446
	19.4719223
Log. sin. 39°. 57′. 37″..........	9.8077084
Log. tan. (24°. 46′ +)	9.6642139
Correspondent log. sine	9.6222792
Which subtracted from above half-sum, gives	9.8496431

= log. sine 45°. 1′. 15″. 15. half the true distance.

Or, by the Third Rule. — by the third rule;

As. comp. log. sin. 44°. 59′. 3″. ..	0.1506351
Log. sine 39°. 50′.	9.8065575
Log. cos. A. (= 25°. 1′. +)	9.9571926
Log. cos. 84°. 51′. 30″...........	8.9523977
4°. 56′. 16″...........	9.9983852
	2)18.9507829
	9.4753914
Log. sin. 39°. 50′.	9.8065575
Log. tan. A..................	9.6691375
1st Sum.	28.9510864

Log. cos. 84°. 46′.	8.9600517
5°. 6′.	9.9982772
	2)18.9583289
	9.4791644
Log. sin. 39°. 57′. 37″............	9.8077084
2d Sum	19.2868728
Diff. sums = log. tan. B. (=24°.46′. +)	9.6642136
As. comp. correspond. log. cos. ..	0.0419345
Log. sin. 39°. 57′. 37″............	9.8077084
Sum=log. sin. 45°. 1′.15″.15 as before	9.8496429

Or, by the Fourth Rule.

—by the fourth rule.

As. comp. log. sin. 44°. 59′. 3″....	0.1506351
Log. sin. 39°. 50′.	9.8065575
Log. cos. A. (= 25°. 1′. +)....	9.9571926
Log. sin. A.	9.6263301
Diff. bet. it and the above as comp.	9.4756950
	2
	18.9513900
Ar. comp. log. cos. 84°. 46′.	1.0399483
5°. 6′.	0.0017228
Log. cos. 84°. 51′. 30″.	8.9523977
4°. 56′. 16″.	9.9983852
	2)38.9438440
Half-sum....	19.4719220
Log. sin. 39°. 57′. 37″..........	9.8077084

Log. tan. (24°. 46′. +)	9.6642136
Correspondent log. sine, which subtract from above half-sum	9.6222790
Diff.=log. sin. 45°. 1′. 15″. 15. half the true distance, as before	9.8496430

The advantages of the preceding modes of reduction are, that they are not difficult in practice, that they are perfectly correct, that they may be applied without using any but the common tables, that they are not incumbered with any complex distinction of cases, and that their results are void of ambiguity. Advantages of these rules.

16th Dec. 1805. Q.

SCIENTIFIC NEWS.

National Institute of France.

The Class of Mathematical and Physical Sciences of the National Institute of France held its public session on the 7th of July last. The order of the readings was as follows: National Institute of France, July 7, 1806.

1. The mathematical prize proposed for the month of January 1809 was announced.

2. A notice of the proceedings of the Class, from the 1st Messidor in the year XIII. to the 1st July 1806, philosophical department, was read by M. Cuvier, the perpetual secretary.

2. A like notice of the mathematical part of the Class during the same interval was read by M. Delambre, perpetual secretary.

4. A memoir on the affinities of bodies for light, by M. Biot.

5. A memoir on the adhesion of the particles of water to each other. By the Count of Rumford, foreign associate.

6. Historical Eulogium on M. Cells. By C. Cuvier.

The

The subject of the mathematical prize, and the prospective remarks upon the same, were as follow:

Prize question. *It is required to establish a theory of the perturbations of the planet Pallas, discovered by Mr. Olbers.*

On the computations for determining the respective places of the last discovered planets.

Geometers have given the theory of perturbations sufficiently extensive and accurate for all the planets formerly known, and for all those which might be discovered, provided they were confined to the same zodiac and had little eccentricity.

Mercury until our time was the most eccentric of all the planets, and at the same time that which had the greatest inclination; but its small mass, and its situation at one of the limits of the planetary system, render it of little effect to produce any sensible alterations in the motions of the other planets; Uranus, discovered twenty-five years ago by Dr. Herschell, is placed at the other limit of the system. With a small mass and moderate eccentricity it has also the smallest of all the known inclinations; so that the formulas which had served for Jupiter and Saturn have been more than sufficient for this modern planet.

Ceres, discovered five years ago by Mr. Piazzi, having with a considerable eccentricity an inclination 10° 38′, must be subject to great and numerous inequalities. It appears, nevertheless, that all the astronomers who have laboured to determine them have been content with the known formulas, of which the developement does not exceed the products of three dimensions of the eccentricities and inclinations. Those of five dimensions have been used in the Mécanique Céleste according to a formula of Mr. Burckhardt. The same astronomer has since presented the general and complete developement of the third, fourth, and fifth orders; but this degree of precision is not sufficient for Pallas, of which the eccentricity is greater than even that of Mercury, and the inclination 34° 38′ is five times as much as that of any antient planet. It is even difficult to conjecture what may be the powers and what may be the dimensions of the products which admit of being neglected; so that the calculations may be so long, and the formulas so complicated

cated as to discourage geometers and astronomers the best qualified to execute a work of this kind.

Two years ago the Class of Physical and Mathematical Sciences of the Institute determined, from this consideration, to propose the subject for the prize to be distributed at the public sitting on the first Monday in Messidor of the year XIV. But the term having appeared too short, and the number of the planets being again increased by the discovery of Juno by M. Harding, of which the eccentricity appears to be still greater than that of Pallas, and the inclination of 13 degrees greatly exceeds that of all the other planets except Pallas; the Class has thought proper to propose the same subject again, with some modifications and a double prize. They accordingly invite astronomers and geometers to discuss completely all the points of this theory, with the omission of none of the inequalities which may become sensible; and as these inequalities cannot be well determined if the elliptical elements be not perfectly known, it is indispensible that the concurrents should not confine themselves to give the numerical coefficients of the equations. It is more particularly important to exhibit analytical formulas, in order that substitution may be successively made of more exact values of the mean distance, the eccentricity, the perihelium, and the inclination, accordingly as the elements shall become better known. The concurrents may even dispense with giving any numerical value, provided the analytical expressions be presented sufficiently in detail to enable an intelligent calculator to follow the developement and reduce them into tables.

Another advantage will result from these general formulas; namely, that the planets Ceres, Pallas, and Juno being at distances from the sun so little different that it can scarcely at present be with certainty decided which of the three is the nearest or the most remote: the formula given for Pallas may serve equally for the two others, as well as for every planet which may hereafter be discovered which shall have its eccentricity and inclination within the same limits.

The Class entertains the hope that this question will appear of sufficient interest to geometers to induce them

to make exertions proportioned to the difficulty of the subject. The prize which will be proclaimed in the public sitting of the first Monday in January 1809 will be a gold medal of 6000 francs (£250).

The works presented must be written either in French or in Latin, and will not be received later than October 1, 1808. This term will be strictly attended to. The other conditions are as usual.

Nitrate of Soda.

Nitrate of soda burns three times as long as common nitre, &c.

Professor Proust writes to Dr. Delamethere, that he finds the nitrate of soda an economical article for fire works. Five parts of the nitrate, one of charcoal and one of sulphur, afford a powder which gives a flame of a reddish yellow, of considerable beauty: and the mixture burned in a metallic tube, will last exactly three times as long as the same charge of common powder.

The nitric acid in this combination is not decomposed to the same degree as that of nitrate of potash. Its gases are a mixture of carbonic acid, with a small quantity of gaseous oxide of azote, and much nitrous gas.

The cheapest method of obtaining nitrate of soda, would no doubt be to use soda, instead of potash, to saturate the mother waters.

Examination of the Birds' Nests which are eaten in China, and other Eastern parts.

Birds' nests of the East.

The same chemist has examined the birds' nests of the East and finds them to consist merely of a single piece of cartilage, uniform in its texture. He boiled one in water, which became soft, but was not separated in its parts and what was still more remarkable, it lost only four hundredths of its weight.

Subterraneous Road or Tunnel, made upwards of three Centuries ago (Journal des Mines, Fev. 1806).

Subterraneous passage or tunnel in Italy, made in the fifteenth century.

The Marquis (de Saluces) Louis II. being desirous of increasing the commerce of the country dependant on his sovereignty, undertook in the fifteenth century to make an excellent road in the valley of the Po, which passing

passing over a mountain, placed beside Mount Viso, called La Traversetta, should lead into Dauphiny.

But as this passage was surrounded by frightful precipices and was only passable for men on foot, he dug through the body of the mountain, a passage, which, without the assistance of gunpowder, was completed in less than five years. This passage is 74 metres (about 80 yards) in length, four in width, and abont the same in height.

The opening through this mountain has been attributed by some to the ancient Romans, at the time when they penetrated into Gaul; others have ascribed it to the celebrated warrior of Carthage, who made the Romans tremble, and was their eternal enemy. But it is certain that it was effected by the Marquis Louis II. The acts relating to several undertakings, composing part of this work, still exist in the archives of the former office of Secretariat of the interior of Piedmont, and Mr. Bresli, sub-prefect of the Arrondissement, author of Notices Historiques de la Ville de Saluces, published at Turenne in the year XIII. asserts that he himself being occupied on the spot in clearing this passage from rocks and other obstacles which had detached themselves from the mountain, observed on the right hand within the same passage, the engraved date of 1480, the epocha at which this work was finished.

Method of conveying Carp and Pike to great Distances alive.

This method which is no less simple than easy, and which I am informed is also practised in England, is mentioned in La Revue. It may be practised by any proprietor of ponds, and may afford a good return if used in situations where carriage may easily be had. The fish it is said, may be thns conveyed some hnndreds of miles, in a state of life and health equal to what they possessed when first caught. Carp and Pike.

Crumb of bread is soaked in brandy, and when well swelled, it is used to fill the whole of the fish's mouth, into which, half a glass more of the spirit is then to be poured. The fish remains motionless and as if deprived of life;

life; in which state it is to be wrapped in fresh straw, and afterwards in a cloth.

In this condition the fish may be kept or conveyed to any distance for eight or ten days. When arrived at the place of destination, they must be unpacked and thrown into a cistern of water, where they remain a quarter of an hour, or sometimes and hour, without shewing any signs of life; but at the end of that time they disgorge very abundantly and recover their life and ordinary motions.

Horse-Chesnuts as Food for Sheep.

Horse Chesnuts for Sheep.

The fruit of the horse-chesnut tree is collected in Saxony for feeding sheep, where it is considered as an wholesome food and a specific remedy against the rot. It is given to them in Autumn when the green food is no longer to be had. The horse chesnuts are cut in pieces and distributed in the quantity of about two pounds and a half for each, and less for the lambs. Sheep as well as cattle at first refuse it, but greedily take it when custom has made it familiar. They eat the prickly outside with satisfaction. There is danger in giving these fruits without cutting them in pieces, as they may stick in the throat and occasion the death of the animal.

Mr. CUTHBERTSON, No. 54, Poland Street, Philosophical Instrument Maker, and Member of the Philosophical Societies of Holland and Utrecht, has in the Press his Work on Practical Electricity and Galvanism; being a Translation of the most interesting Experiments contained in a Treatise published by him during his residence in Holland, with the addition of all such as have since been invented by Himself and Others; together with an Appendix, containing the most interesting Experiments on Galvanism.

A
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AND
THE ARTS.

DECEMBER, 1806.

ARTICLE I.

Facts and Observations respecting Vision under Water; and various Particulars relative to Swimming, &c. In a Letter from James Horsburgh, *Esq.*

To Mr. NICHOLSON.

Sir, Walworth, Nov. 6, 1806.

THE perusal of your observations relative to Swimming, in No. 58 of your Journal, and the letters from your correspondent inserted in Nos. 60 and 61, affirming that objects are visible to the human eye under water, which concurs not with your opinion, encourage me to state some few facts relative to this subject, the result of my own observations.

On swimming, and vision under water.

In high latitudes the sea is seldom transparent; but within the tropics, and near the equator, the bottom is often visible in from ten to fifteen or twenty fathoms water, when it consists of variegated coral or white sand and coral mixed. In various parts of the Indian seas, the

Great transparency of the sea between tropics. Bottom seen at 150 feet depth.

the bottom is discernible at the depths mentioned above. In the Mindora Sea I have seen the spotted coral at the bottom when the depth of water over it was twenty-five fathoms; and have often tacked ship on the edges of coral shoals, by the appearance of the bottom, in depths of ten and twelve fathoms.

The author can see objects with the face under water nearly as well as in the air.

Swimming has ever been my favourite amusement. At Madras, where the surf is high, and ships' boats not permitted to land, I once had the imprudence to swim on shore through the surf, by way of diversion, and returned through it to a boat at a grapnel outside, which nearly cost me my life, being obliged to dive so frequently in resisting many heavy surfs. I have always observed when the bottom was clear, and any object upon it visible when my head was above water, it was nearly as well perceived when my head was under water. At the island Diego Garcia, where the water is very transparent in the harbour and at other places, I have frequently, when swimming under water, seen fishes darting about in various directions, and every article on the bottom very plain. The legs and feet of persons standing at six or eight yards distance were always visible in clear water when my head was under it; and at discretion easily taken hold of, by swimming under water to them with open eyes.

As far as his experience goes, all persons can see under water

I have always supposed that all persons could see under water, if timidity did not prevent them from opening their eyes during immersion; but must own that I have been in company with persons who could swim, but could not be prevailed on to open their eyes under water, affirming that they could not, although they endeavoured to do it. Those persons who could open their eyes with facility whilst immersed, always asserted that they saw objects in the water. Vision under water is probably confined to this element, for I never could distinguish external objects, such as the sun, clouds, &c. but only confused rays of light (in looking upward) were visible.

— but not objects in the air, while themselves are immersed.

Infants probably will float.

It is probable that most infants will float; I have seen one of ten or twelve months old fall from a boat into the water; the mother leaped in and rescued it, apparently without either receiving injury. The natives of China that

that live in boats do not, however, trust to their children floating naturally, for they keep gourds fixed to their shoulders to prevent them sinking should they happen to fall in the water. This precaution seems prudent in China, the water in the canals and rivers here being of a very soft quality.

Men are apt to drown themselves by raising their hands.

Men who cannot swim, happening to fall in the water, are certainly (as you observe) very apt to drown themselves by pushing their hands and arms above water; for a person cannot sink if the hands are kept under the surface and a gentle motion performed by them. But I cannot avoid observing that Doctor Franklin's remarks relative to the specific gravity of the human body in salt and fresh water seem liable to some objections, if indiscriminately applied to all persons, there being great reason to think the specific gravity of the human species differs considerably. The following instance appears to favour this opinion: In company with two friends, some years ago, it was customary for us to proceed to the sea to enjoy the exercise of swimming; this was at Bombay in India; the gentlemen who accompanied me could both swim, but neither of them had ever tried to float on the water without motion. When swimming on their backs they were requested to endeavour to lie quiet without any motion of hands or feet. The best swimmer of the two could not float without using a little motion with his hands or feet, although he repeated the trial several days; when he lay without motion his head gradually sunk till completely under water. This person was of short stature, strong, and athletic. The other gentleman was of a spare make, thin, and delicate in constitution; and at the first trial floated on the surface like a cork, without any motion of hands or feet; his toes, part of his feet, knees, part of his shoulders and head, remained above the surface, when every part of his body was at rest; whilst the stout gentleman could in no position float on the surface without a gentle motion of hands or feet. It certainly appeared to me evident that the specific gravity of these persons differed considerably.

Facts which shew that the specific gravities of men, and consequently their powers of floating, are different.

Particular observations respecting the

When the sun has been obscured by clouds, or otherwise not too powerful, I have frequently amused myself by

situation of floating persons, &c.

by lying on my back without motion for considerable periods of time; sometimes for half an hour, or longer, when the water was mild and smooth; at such times I have found a strong inclination to sleep, which induced me to lie no longer without motion, for fear of carrying this amusement too far. When floating on salt water I have always observed the following effects produced by placing the arms in various positions: 1st. When my arms were placed across my breast, the body floating in the horizontal position, face upwards, in a short time the feet and legs sunk downward, until the body assumed nearly a vertical position; then the head frequently sunk so far as to bring the nose under the surface, but the face quickly resumed its former position above the surface without using the least motion of any limb. The body alters its position, sometimes, by turning round from one side to the other when the feet sink far below the surface, but soon returns to its natural floating position with the back undermost, the legs and feet at the same time ascending to the surface as at first. With the hands laid across the breast this revolution of the body in floating on the sea has been reiterated often in the space of a quarter of an hour without moving a limb.

2nd. When the arms were laid close along each side, over the belly, or under the back, the body was liable to the same revolutions as mentioned above.

3rd. When the arms were stretched in a perpendicular direction from the body, they always prevented it from turning round by acting on the water as levers to retain the body in its natural floating position; although with the arms in this position the feet sometimes descended considerably from the surface, but shortly after ascended to it again.

4th. To keep the body in the horizontal equilibrium, the arms were stretched backward beyond the head, the hands open and resting on the surface of the water; the legs and feet then remained constantly near the surface, the toes generally above it. On drawing the arms from this position gradually forward to the perpendicular direction from the body, the feet always inclined to descend from the surface; but so soon as the arms were moved

moved more backward, the toes and part of the feet always appeared above water, the body continuing in perfect equilibrium, with the face and toes above the surface, the chest and knees close to it. When the water is not too cold, and the surface smooth, it would be an easy matter to fall asleep floating in this position. Particular observations respecting the situation of floating persons, &c.

None but those who can swim above and under water will readily comprehend the great pressure of the water upward, and how easily it will sopport on its surface human beings, when it is smooth, without any broken water. For amusement I have gone into the sea full dressed into deep water, and by floating in various ways, as most convenient, taken off coat, waistcoat, opened the knee buttons of my smallclothes, taken them and stockings off with equal ease as on shore, and finally pulled my shirt off, swimming then with them to the shore.

Swimming is very easily acquired when a few good lessons are given. Seamen, and others who are liable to be much on the sea, rivers, canals, &c. should not neglect to learn this art.

I am, Sir,
Your most obedient,
and most humble servant,
JAS. HORSBURGH.

II.

On the Quantity and Velocity of the Solar Motion. By William Herschel, *L.L.D. F. R. S. From the Philosophical Transactions for* 1806.

[Concluded from p. 242.]

Remarks on the sidereal Motions as they are represented from Observation.

As we have now before us a set of figures which give a complete view of the result of the calculations contained in the Xth Table, we may examine the arrangements of the stars, and draw a few conclusions from them, that will Investigation of the proper motion of the sun.

Investigation of the proper motion of the sun.

will throw some light upon the subject of our present inquiry.

In the first place, then, we have to observe in Fig. 1, that 17 out of the 21 stars, whose motions are directed towards the north, are crowded together into a compass of little more than $76\frac{1}{2}$ degrees. But this figure, as we have shown, is drawn from observation. We are consequently obliged to conclude, that, if these motions are the real ones, there must be some physical cause which gives a bias to the directions in which the stars are moving; if so, it would not be improbable that the sun, being situated among this group of stars, should partake of a motion towards the same part of the heavens.

Our next remark concerns the velocity of the sidereal motions; and therefore we must have recourse to Fig. 2, where we perceive that the greatest motions aro not confined to the brightest stars. For instance, the velocity of β Virginis is but little inferior to that of Arcturns, and exceeds the velocity of Procyon. Likewise the velocities of β Aquilæ, α Libræ, and α Capricorni, surpass that of Sirius; and an inspection of the rest of the figure will be sufficient to show how very far the velocities of Capella, Lyra, Rigel, α Orionis, Aldebaran, and Spica, are exceeded by those of many other stars.

If we look at the arrangement of the stars with respect to the direction of the solar motion, we find in Fig. 3, that a somewhat different scattering of them has taken place; but still most of the stars appear to be affected by some cause which tends to lead them to the same part of the heavens, toward which the sun is moving; and the directions of the greatest number of them are not very distant from the line of the solar motion.

The whole appearance of this figure presents us with the idea of a great compression above the centre, arising from some general cause, and a still greater expansion in the lower part of it. The considerable projection of a few stars on both sides, is however a plain indication that the compressing or dilating cause does not act in their directions.

When the velocity of the stars, represented in the same point of view in Fig. 4, is examined, we find a particularity

ticularity in the direction and comparative velocities in the largest stars that must not be overlooked. Four of them, Rigel, α Orionis, Spica, and Antares, have a motion toward that part of the heavens in which the solar apex is placed, and their motions are very slow. Three other stars of the 1st magnitude, Arcturus, Procyon, and Sirius, move toward the opposite part of the heavens, and their motions, on the contrary, are very quick.

Investigation of the proper motion of the sun.

The direction of the motion of Aldebaran, compared with its small velocity, is no less remarkable; and seems to be contrary to what has been pointed out with the three last mentioned stars; we shall however soon have an opportunity of showing that it is perfectly consistent with the principles of the solar motion.

The Solar Motion and its Direction assigned in the first Part of this Paper are confirmed by the Phenomena attending the observed Motions of the 36 Stars.

An application of some of the foregoing remarks will be our next subject; and I believe it will be found, that in the first place they point out the expediency of a solar motion. That next to this they also direct us to the situation of the apex of this motion: and lastly, that they will assist us in finding out the quantity requisite for giving us the most satisfactory explanation of the phenomena of the observed proper motions of the stars.

In examining the second figure, it has been shown that no less than six stars of the first magnitude, namely, Capella, Lyra, Rigel, α Orionis, Aldebaran, and Spica, have less velocity than nine or ten much smaller stars. Aldebaran and α Orionis indeed have so little motion that there are but three stars in all the 36 that have less. But the situation of these bright stars, from their nearness, must be favourable to our perceiving their real motions if they had any, unless they were counteracted by some general cause that might render them less conspicuous. Now to suppose that the largest stars should really have the smallest motions, is too singular an opinion to be maintained; it follows, therefore, that the apparently small motions of these large stars is owing to some gene-

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Investigation of the proper motion of the sun.

ral cause, which renders at least some part of their real motion invisible to us. But when a solar motion is introduced, the parallax arising from that cause will completely account for the singularity of these slow motions.

If the foregoing argument proves the expediency of a solar motion, its direction is no less evidently pointed out by it. For if the parallax occasioned by the motion of the sun is to explain the appearances that have been remarked, it will follow, that a direction in opposition to the motion of Arcturus, will answer that end in the most satisfactory manner. That compression, for instance, which has been remarked in the motions of the stars moving toward the solar apex in Fig. 3, and which is so completely accounted for by a parallactic motion arising from the motion of the sun, points out the direction in which the sun should move, in order to produce this required parallactic motion. The expansion of the motions that are in opposition to the former is evidently owing to the same parallactic motions, which in this direction unite with the real motions of the stars; and as, in the former case, the observed motions are the differences between the parallactic and real motions, so here they are the sum of them.

The remark that stars having a side motion, are not affected by the cause of the compression or expansion, which acts upon the rest, is perfectly explained; for a parallactic motion, in the direction of the motion of Arcturus, can have no effect in lengthening or shortening the perpendicular distance to which a star may move in a side direction.

I have only to add, that the small velocities of Rigel, α Orionis, Spica, and Antares, in Fig. 4, as well as the great velocities of Arcturus, Procyon, and Sirius, point out the same apex which in the first part of this Paper has already been established by more extended computations.

The case of Aldebaran, though seemingly contrary to what has been shown, confirms the same conclusions. This will appear by considering that a star, moving toward the solar apex with a greater real motion than its parallactic one, must continue apparently to move in its real

real direction; but should a star, such as Aldebaran, move toward the apex with less velocity than the parallactic motion which opposes it, there will arise a change of direction, and the star will be seen moving toward the opposite part of the heavens. Investigation of the proper motion of the sun.

Trial of the Method to obtain the Quantity of the Solar Motion by its Rank among the sidereal Velocities.

According to the conditions that have been explained, a calculation may be made with a view of equalizing the velocities of the sun and the star α Orionis; and the result of it will show that the proposed equality will be obtained when the solar motion is 1″,266230. It will moreover be found that so small an increase of this motion as 0″,01 would give us 19 stars with less, and 17 with more velocity than that which the calculation assigns to the sun; this consequently fixes one of the limits to which the solar motion ought not to come up, if we intend it should hold a middle rank among the sidereal velocities.

On the other hand, by a similar calculation of the velocities of the star Pollux and the sun, it appears that a solar motion of 0″,967754 will make them equal; and that a diminution of this motion not exceeding 0″,01 would give us 19 stars moving at a greater rate than the sun, and only 17 falling short of its velocity. This consequently fixes the other limit to which the solar motion ought not to be depressed. And thus it appears by this method, that the quantity we are desirous of ascertaining, is confined within very narrow bounds, and that by fixing upon a mean of the two limits, we may have the rank of the solar motion true to less than 0″,15.

Calculations for investigating the Consequences arising from any proposed Quantity of Solar Motion, and for delineating them by proper Figures.

Before we can justly examine the real motions of stars which it will be necessary to admit in consequence of a given solar motion, it will be convenient to have them represented in two figures that we may see their arrangement

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ment and extent; and as a calculation of the require particulars will oblige us to fix upon a certain quantity we shall take the motion that has been ascertained to be long to the middle rank of the sidererl velocities for a pattern. The result of the necessary calculations is a follows:

Table XI.

Names.	Parallactic Motion.	Real Motion.	Parallactic Angle.		Velocity.
Sun	0″,00000	1″,116992	0°0.0′0.0″		1116992
Sirius	0,75697	0,395212	149.20. 6	*sf*	395212
Arcturus : ..	0.59847	1,488713	179.59.55.7	*sp*	1786455
Capella.	0,88905	0,506123	22.29.12,5	*nf*	632654
Lyra.	0,36349	0,498949	40.29.14	*nf*	648634
Rigel	0,55470	0,700381	4.36.52	*np*	957665
αOrionis....	0,71410	0,842559	1.38.38	*np*	1137455
Procyon....	0,74161	0,523428	156.32.21	*sp*	732799
Aldebaran ..	0,72736	0,608148	2.45.15	*nf*	851407
Pollux	0,78643	0,743971	50.12.11	*np*	1056139
Spica.	0,74009	0,902004	7. 6.44	*np*	1298886
Antares	0,71110	1,000835	0.16.10,5	*np*	1461219
Altair	0,64514	1,071042	40.48. 4	*nf*	1574431
Regulus	0,75095	0,706833	17.43.53	*np*	1046113
βLeonis	0,68003	0,443842	54.10.14,5	*np*	665763
βTauri.	0,73063	0,633317	2. 5.15,5	*nf*	949976
Fomalhaut..	0,66693	0,383414	13.22. 5,5	*nf*	575121
αCygni.	0,46516	0,529503	0.18. 2,2	*np*	847204
Castor	0,55841	0,474647	11.30.32	*np*	949293
αOphiuchi ..	0,35202	0,290934	8.23.43	*nf*	581869
αCoronæ ...	0,23427	0,370580	37.21.17	*nf*	741160
αAquarii ...	0,55743	0,756754	4.38.19,5	*nf*	1513508
αAndromedæ	0,55389	0,464035	2.33.34	*nf*	928071
αSerpentis ..	0,38655	0,598458	6.38.54	*nf*	1196917
αPegasi	0,55567	0,734265	5.35.47,5	*nf*	1468530
αHydræ	0,46554	0,538281	17. 8.26	*np*	1238046
α^2 Libræ	0,43377	0,563892	15. 4.29	*np*	1353342
γPegasi.	0,44540	0,618272	1.39.27	*nf*	1545679
αArietis	0,43893	0,342934	9.35.29,5	*nf*	857336
αCeti	0,33271	0,454165	11.26. 5,5	*np*	1271662
αHerculis. ..	0,21909	0,446795	5.56.38,5	*nf*	1340388
βVirginis ..	0,36039	0,967572	48.29. 2,5	*nf*	2902716
γAquilæ....	0,30898	0,502168	0.36.25	*nf*	1506503
α^2 Capricorni	0,31390	0,537285	19.51.52.5	*nf*	1880497
βAquilæ....	0,24370	0,226158	96.36.59,5	*sp*	905830
α' Capricorni.	0,26151	0,519230	17. 4.54,5	*nf*	2180769
α' Libræ	0,17347	0,349371	26.29.44,5	*np*	2096229

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By the contents of this Table, Fig 5 is drawn with the lines contained in the third column and the angles of the fourth; the scale of it is that of the 5th and 3d figures; and it represents the directions and angular quantities of the real motions that are required to compound with the parallactic effects of the second column, so as to produce those annual proper motions which are established by observation.

Fig. 6 is drawn on the reduced scale of the 2d and 4th figures. The lines make the same angles with the direction of the solar motion as before, but their lengths are in the proportion of the velocities contained in the last column.

Remarks that lead to a necessary Examination of the Cause of the sidereal Motions.

The first particular that will strike us when we cast our eye on Fig. 5, is the uncommon arrangement of the stars. It seems to be a most unaccountable circumstance that their real motions should be as represented in that figure; indeed, if we except only ten of the stars, all the rest appear to be actuated by the same influence, and, like faithful companions of the sun, to join in directing their motions towards a similarly situated part of the heavens.

This singularity is too marked not to deserve an examination; for unless a cause for such peculiar directions can be shown to exist, I do not see how we can reconcile them with a certain equal distribution of situations, quantities, and motions, which our present investigation seems to demand. In order to penetrate as far as we can into this intricate subject, we shall take a general view of the causes of the motions of celestial bodies.

A motion of the stars may arise either from their mutual gravitation toward each other, or from an original projectile force impressed upon them. These two causes are known to act on all the bodies belonging to the solar system, and we may therefore reasonably admit them to exert their influence likewise on the stars. But it will not be sufficient to know a general cause for their motions,

tions, unless we can show that its influence will tend to make them go toward a certain part of the heavens rather than to any other. Let us examine how these causes are acting in the solar system. Investigation of the proper motion of the sun.

The projectile motions of the planets, the asteroids, and the satellites, excepting those of the Georgium Sidus, are all decidedly in favour of a marked singularity of direction. We may add to them the comet of the year 1682, whose regular periodical return in 1759 has sufficiently proved its permanent connection with the solar system. Here then we have not less than 23 various bodies belonging to the solar system to show that this cause not only can, but in the only case of which we have a complete knowledge, actually does influence the celestial motions, so as to give them a very particular appropriate direction. Even the exception of the Georgian satellites may be brought in confirmation of the same peculiarity; for though they do not unite with the rest of the bodies of our system, they still conform among each other to establish the same tendency of a similar direction in their motion round the primary planet. And thus it is proved that the similar direction of the motion of a group of stars may be ascribed to their similar projectile motions without incurring the censure of improbability.

Let us however pursue the objection a little farther, and as we have shown that the celestial bodies of the solar system actually have these similar projectile motions, it may be required that we should also prove that the stars have them likewise; since the appearances in Fig. 5 may otherwise be looked upon as merely the consequence of the assumed solar motion. To this I answer, that setting aside the solar motion, and allowing the observations of astronomers on the proper motion of the stars to give us the real direction and angular quantity of these motions, even then the same similarity will equally remain to be accounted for. In my examination of Fig. 1 and 3, it has been shown that we ought to ascribe the similar directions of the sidereal motions to some physical cause, which probably exerts its influence also on the solar motion; therefore in reverting to those figures I may

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I may be said to appeal to the actual state of the heavens, for a proof of what has been advanced, with respect to the similarity of the directions of projectile motions.

Having thus examined one cause of the sidereal motions, and shown that as far as we are acquainted with its mode of acting in the solar system, it is favourable to a similariy of direction; and that moreover, if we ascribe the motion of the stars to it, we have also good reason, from observation, to believe it to be in favour of the same similarity; we may in the next place proceed to consider the mutual gravitation of the stars toward each other. This is an acknowledged principle of motion, and the laws of its exertion being perfectly known, we shall in this inquiry meet with no difficulty relating to its direction, which is always toward the attracting body.

Considerations of the attractive Power required for a sufficient Velocity of the sidereal Motions.

As attraction is a power that acts at all distances, we ought to begin by examining whether the motions of our stars can be accounted for by the mutual gravitation of neighbouring stars toward each other, or by a periodical binal revolution of them abont a common centre of gravity; or whether we ought not rather to have recourse to some very distant attractive centre. This may be decided by a calculation of the effects arising from the laws according to which the principle of attraction is known to act. For instance, let the sun and Sirius be two equal bodies placed in the most favourable situation to permit a mutual approach by attraction: that is, let them be without projectile motions, and removed from all other stars which might impede their progress toward each other, by opposite attractions. Then, by calculation, the space over which one of them would move in a year, were the matter of both collected in the other as an attractive centre, would be less than a five thousand millionth part of a second; supposing that motion to be seen by an eye at the distance of Sirius, and admitting the parallax of the whole orbit of the earth on this star to be one second.

This

This proves evidently that the mere attraction of neighbouring stars acting upon each other cannot be the cause of the sidereal motions that have been observed.

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In the case of supposed periodical binal revolutions of stars about a common centre of gravity, where consequently projectile motions must be admitted, the united power of the connected stars, provided the mass of either of them did not greatly exceed that of the sun, would fall very short of the attraction required to give a sufficient velocity to their motions. The star Arcturus, for example, which happens to move, as is required, in an opposite direction to the proposed solar motion, were it connected with the sun, and had the proper degree of necessary projectile motion, could not describe an arch of 1″ of its orbit, about their common centre, in less than 102 years; and though the opposite motion of the sun, by a parallactic effect would double that quantity, it still would fall short of the change we observe in this star in the course of a single year.

Other considerations are still more against the admission of such partial connections: they would entirely oppose the similarity of the directions of the sidereal motions that have been proved to exist, and which we are now endeavouring to explain.

Let us then examine in what manner a distant centre of attraction may be the cause of the required motions. By admitting this centre to be at a great distance, we shall have its influence extended over a space that will take in a whole group of stars, and thus the similar directions of their motions will be accounted for. Their velocities also may be ascribed to the energy of the centre, which may be sufficiently great for all the purposes of the required motions. A circumstance, however, attends the directions of the motions to be explained, which shows that a distant centre of attraction alone will not be sufficient; for these motions, as we may see in Fig. 3, though pretty similar in their directions, still are diverging; whereas if they were solely caused by attraction, they would converge toward the attracting centre, and point out its situation. It is therefore evident that projectile motions must be combined with attraction, and that the motions

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motions of the stars when regulated in this manner, are not unlike the disposition by which the bodies of the solar system are governed. If we pursue this arrangement, it will be proper to consider the condition, and probable existence of such a centre of attraction.

There are two ways in which a centre of attraction, so powerful as the present occasion would require, may be constructed: the most simple of them would be a single body of great magnitude; this may exist, though we should not be able to perceive it by any superiority of lustre; for notwithstanding it might have the usual starry brightness, the decrease of its light arising from its great distance would hardly be compensated by the size of its diameter; but to have recourse to an invisible centre, or at least to one that cannot be distinguished from a star, would be entirely hypothetical, and, as such, cannot be admitted in a discussion, the avowed object of which is to prove its existence.

The second way of the construction of a very powerful centre, may be joint attraction of a great number of stars united into one condensed group.

The actual existence of such groups of stars has already been proved by observations made with my large instruments; many of those objects, which were looked upon as nebulous patches, having been completely resolved into stars by my 40 and 20-feet telescopes. For instance, the nebula discovered by Dr. Halley in the year 1714, in which the discoverer, and other observers after him, have seen no star, I have ascertained to be a globular cluster, containing, by a rough calculation, probably not less than fourteen thousand stars. From the known laws of gravitation, we are assured that this cluster must have a very powerful attractive centre of gravity, which may be able to keep many far distant celestial bodies in control.

But the composition of an attractive centre is not limited to one such cluster. An union of many of them will form a still more powerful centre of gravitation, whose influence may extend to a whole region of scattered stars. To prove that I argue entirely from observations, I shall mention that another nebula, discovered by Mr.

Mr. Messier in the year 1781, is, by the same instruments, also proved to consist of stars; and though they are seemingly compressed into a much smaller space, and have also the appearance of smaller stars, we may fairly presume that these circumstances are only indications of a greater distance, and that, being a globular cluster, perfectly resembling the former, the distance being allowed for, it is probably not less rich in the number of its component stars. The distance of these two clusters from each other is less than 12 degrees, and we are certain that somewhere in the line joining these two groups there must be a centre of gravitation, far superior in energy to the single power of attraction that can be lodged in either of the clusters.

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I have selected these two remarkable objects merely for their situation, which is very near the line of the direction of the solar motion; but were it necessary to bring farther proof of the existence of combined attractions, the numerous objects of which I have given catalogues* would amply furnish me with arguments.

If a still more powerful but more diffused exertion of attraction should be required than what may be found in the union of clusters, we have hundreds of thousands of stars, not to say millions, contained in very compressed parts of the milky way, some of which have already been pointed out in a former paper †. Many of these immense regions may well occasion the sidereal motions we are required to account for; and a similarity in the direction of these motions will want no illustration.

With regard to the situation of the condensed parts of the milky way, and of the two clusters that have been mentioned, we must remark, that the seat of attraction may be in any part of the heavens whatsoever; for when projectile motions are given to bodies that are retained by an attractive centre, they may have any direction, even that at right angles to its situation not excepted.

* Phil. Trans. for 1786, page 457; for 1789, page 212; for 1802, page 477.

† Ibid. for 1802, page 495.

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It will give additional force to the arguments I have used for the admission of far distant centres of attraction, as well as projectile motions in the stars that are connected with them, when we take notice that, independent of the solar motion, and setting that entirely aside, the action of these causes will be equally required to explain the acknowledged proper motions of the stars. For if the sun be at rest, then Arcturus must actually change its place more than 2″ a year, and consequently this and many other stars, which are well known to change their situation, must be supposed to have projectile motions, and to be subject to the attraction of far distant centres.

Determination of the Quantity of the Solar Motion.

If I am not mistaken, it will now be allowed that no objection can arise against any solar velocity we may fix upon, for want of a cause that may be assigned to act upon the sun, and many stars, so as to account for their motions, and similar tendency toward a certain part of the heavens; we may consequently proceed in examining whether the quantity that has been assumed for calculating the contents of the XIth Table, will sufficiently come up to the conditions we have adopted for directing our determination.

In Fig. 6 we have the velocities of the 36 stars delineated, and by examining the last column of the Table from which they are taken, we find that the parallactic effects arising from the proposed solar motion require the velocity of 18 stars to exceed that of the sun, and exactly the same number to be inferior to it; so far then the rank which has been assigned to the solar motion is a perfect medium among the sidereal velocities.

If we examine in the next place how this motion will agree with a mean rate deduced from the velocities in the above mentioned column, we find a 36th part of their sum to be 1196550. A solar motion, therefore, which agrees with this mean rate will differ from one assigned by the middle rank no more than 0″,079558; and, on account of the smallness of this quantity, the calculations required to lessen it, by some little increase of the solar motion,

motion, might well be dispensed with; but if we were desirous of greater precision, the secondary purpose, next to be considered, would rather incline us to an opposite alteration.

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The great disparity of the sidereal motions, which has been mentioned as an incongruity in the first part of this Paper, and has more evidently been shown to exist when we examined the representations of these motions in the 3d figure, is the next point we have to consider in the effect of the solar motion. Let us see how far we have been successful in lessening the ratio these velocities bear to each other. The last column of the Xth Table contains them as they must have been admitted if the sun had been at rest. The proportion of the quickest motion to the slowest is there as 2501621 to 103036; and the velocity of one is therefore above 24 times greater than that of the other. But in consequence of the solar motion we have used, the two extreme velocities are reduced to 2902716 and 395212; which gives a proportion of less than 7¼ to 1.

If the quantity of the solar motion were lessened to 1″, we might bring the ratio of the extreme velocities so low as 6 to 1; but as the middle rank has already given it a little below the mean rate, I do not think that we ought to lower it still more; so that when all circumstances are properly considered, there is a great probability that the quantity assumed in the last calculation may not be far from the truth. It appears, therefore, that in the present state of our knowledge of the observed proper motions of the stars, we have sufficient reason to fix upon the quantity of the solar motion to be such as by an eye placed at right angles to its direction, and at the distance of Sirius from us, would be seen to describe annually an arch of 1″,116992 of a degree; and its velocity, till we are acquainted with the real distance of this star, can therefore only be expressed by the proportional number of 1116992.

Concluding Remarks and Inferences.

We have now only to notice a few remarks that may be

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made, by way of objection to the solar motion I have fixed upon. If the quantity of this motion is to be assigned by the mean rank of sidereal velocities, it may be asked, will not the addition of every star, whose proper motion shall be ascertained, destroy that middle rank, which has been established? To this I shall answer, that future observations may certainly afford us more extensive information on the subject, and even show that the solar motion should not exactly hold that middle rank, which from various motives we have been induced to assign to it; but at present it appears, that according to the doctrine of chances, a middle rank among the sidereal velocities must be the fairest choice, and will remain so, unless, what is now a secondary consideration, should hereafter become of more importance than the first. That this should happen is not impossible, when a general knowledge of the proper motions of all the stars of the 1st, 2d, and 3d magnitudes can be obtained; but then the method of calculation that has been traced out in this and the former Paper, is so perfectly applicable to any new lights observation may throw upon the subject, that a more precise and unobjectionable solar motion can be ascertained by it with great facility. Hitherto we find that a mean rank agrees sufficiently with the phenomena that were to be explained: the apparent velocities of Arcturus and Aldebaran, without a solar motion for instance, were to each other, in the IXth Table, as 208 to 12; our present solar motion has shown, that when the deception arising from its parallactic effect is removed by calculation, these velocities are to each other only as 179 to 85, or as 2 to 1. And though Arcturus still remains a star that moves with great velocity, yet in the XIth Table we have 4 or 5 stars with nearly as much motion; and 4 with more.

Our solar motion also removes the deception by which the motion of a star of the consequence of α Orionis is so concealed as hardly to show any velocity; whereas by computation we find that it really moves at a rate which is fully equal to the motion of the sun.

I must now observe, that the result of calculations founded upon facts, such as we must admit the proper motions

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motions of the stars to be, should give us some useful information, either to satisfy the inquisitive mind, or to lead us on to new discoveries. The establishment of the solar motion answers both these ends. We have already seen that it resolves many difficulties relating to the proper motions of the stars, and reconciles apparent contradictions; but our inquiries should not terminate here. We are now in the possession of many concealed motions, and to bring them still more to light, and to add new ones by future observations should become the constant aim of every astronomer.

This leads me to a subject, which though not new in itself, will henceforth assume a new and promising aspect. An elegant outline of it has long ago been laid before the public in a most valuable paper on general Gravitation, under the form of "Thoughts" on the subject*; but I believe, from what has been said in this Paper, it will now be found that we are within the reach of a link of the chain which connects the principles of the solar and sidereal motions with those that are the cause of orbitual ones.

A discovery of so many hitherto concealed motions, presents us with an interesting view of the construction of that part of the heavens which is immediately around us. The similarity of the directions of the sidereal motions is a strong indication that the stars, having such motions, as well as the sun, are acted upon by some connecting cause, which can only be attraction; and as it has been proved that attraction will not explain the observed phenomena without the existence of projectile motions, it must be allowed to be a necessary inference, that the motions of the stars we have examined are governed by the same two ruling principles which regulate the orbitual motions of the bodies of the solar system. It will also be admitted that we may justly invert the inference, and from the operation of these causes in our system, conclude that their influence upon the sidereal motions will tend to produce a similar effect; by which means the probable motion of the sun, and of the stars in

* See the note Phil. Trans. for 1783, page 283.

orbits,

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orbits, becomes a subject that may receive the assistance of arguments supported by observation.

What has been said in a paragraph of a former Paper, where the sun is placed among the insulated stars *, does not contradict the present idea of its making one of a very extensive system. On the contrary, a connection of this nature has been alluded to in the same Paper†. The insulation ascribed to the sun relates merely to a supposed binary combination with some neighbouring star; and it has now been proved by an example of Arcturus, that the solar motion cannot be occasioned or accounted for by a periodical revolution of the sun and this or any other star about their common centre of gravity.

III.

Explanation of a common Impurity in the Nitrate of Ammonia, which interferes with the production of Nitrous Oxide. By Mr. John Sadler.

Unexpected ebullition and expansion of the nitrate of ammonia.

In attempting to procure nitrous oxide from nitrate of ammonia by heat, it has frequently happened, when I expected the salt to be at the point of decomposition, and nitrous oxide about to be formed, a violent ebullition has taken place, and dense white fumes have been disengaged so rapidly as frequently to burst the retort. When first I observed the fact, I imagined too great a degree of heat had been employed, and the nitrate of ammonia rapidly sublimed and carried over with the nitrous oxide. In my subsequent attempts I endeavoured to guard against the possibility of falling into the same error by increasing the heat gradually and interposing a plate of iron between the bottom of the retort and the furnace when I supposed the heat too intense; yet, notwithstanding these precautions, the same rapid disengagement of dense white

It did not arise from excess of heat.

* See the note Phil. Trans. for 1802, page 478.

† Phil. Trans. for 1802, page 479.

vapours

vapours took place, and continued for a considerable time after the retort had been removed from the fire. The residue in the retort scarcely ever exhibited the same phenomena upon being again subjected to the same treatment; but nitrous oxide was always prodnced without any farther trouble.

Having frequently procured nitrous oxide without any of the phenomena I have described, I snspected the nitrate of ammonia I had been operating on was impure. I made an examination of the salt, and found it contained a portion of mnriate of ammonia. To ascertain whether the muriatic acid had been the cause of the rapid disengagement of white vapour, I took a portion of the same kind of salt I had before made use of, and freed it perfectly from muriatic acid by means of nitrate of silver. The purified salt I introduced into a retort, and proceeding in the usual way, obtained pure nitrous oxide without any of the dense white vapour.

Suspected impurity.

It was muriate of ammonia.

To assure myself the presence of the muriatic acid had been the occasion of the facts I observed, I made some pure nitrate of ammonia by the direct combination of pure nitric acid and carbonate of ammonia. I took two portions of the solution, and to one of them added a solution of muriate of ammonia. The two solutions were then crystallised, an equal quantity of each salt was exposed to the heat of an Argand lamp, in glass retorts communicating with the pneumatic trough; the retort containing the compound salt gave out very rapidly a dense opake elastic fluid similar to the white vaponrs first described; after a short time the evolution of gas became considerably less abundant, and the interior of the retort became clear; the receiver was then changed; upon examination, what came over now was nearly pure nitrons oxide.

Proof by direct experiment.

The first portions of the gas evolved, that is the opake gas, had the following properties:

Properties of the opake gas obtained from the impure salt,

1st. It remained opake after being passed through cold water.

2nd. A slip of paper coloured blue by tincture of turnsole, when immersed in the gas was changed to red.

3rd. It is not inflammable.

4th. A

4th. A lighted taper is immediately extinguished by immersion in it.

5th. It has a disagreeable acidulous taste, and pungent smell, in which the peculiar odour of nitro-muriatic acid gas is perceptible.

6th. An equal bulk of water did not seem to dissolve any very considerable portion of this gas.

Pure nitrate gives much more of the oxide.

The retort containing the pure nitrate, exposed to the same degree of heat, gave out very soon pure nitrous oxide; but considerably more in quantity; I think considerably above a third more.

It may be observed from what I have detailed, that the presence of muriatic acid is of considerable disadvantage in nitrate of ammonia, when intended for the production of nitrous oxide, the process being rendered by it so much more troublesome, and the quantity of oxide so much less than what is obtained from an equal quantity of pure salt.

I have not observed how small a quantity of muriate affects the process, but considerably under $\frac{1}{8}$ is sufficient to make a disagreeable interruption in the operation.

Farther experiments still wanting.

Many experiments are wanting to point out the peculiar nature of the gas first produced by the decomposition of the impure salt; an examination may probably tend to throw some more light upon the nature of affinities. The subject I think is worth pursuing; at present I have neither the time nor means to give it a farther examination; at some future period I may proceed farther, unless some abler person should take it in hand.

IV.

On the Absorption of Electric Light by different Bodies. In a Letter from Mr. Wm. Skrimshire, Jun. *to* Mr. Cuthbertson, *Philosophical Instrument Maker, Poland Street, Soho. Communicated by Mr. Cuthbertson.*

Dear Sir,

If you think any of the following facts worthy of publicity, you are at liberty to make what use you please of them.

You know that if a shock be passed through or over the surface of a lump of sugar, the electric light is absorbed, and the sugar becomes luminous. This circumstance induced me to try if other substances did not possess the same property, and with this view I have undertaken a course of experiments, beginning with the calcareous substances, which genus I have already gone through as far as circumstances would permit me. Whether or not the same thing has been undertaken by others I do not know, but as far as my reading enables me to decide, I think it has not; at least with the sole view to the phosphorescence of these substances systematically pursued, the path is clearly untrodden.

Electric light renders sugar luminous.

My mode of making the experiments was as follows:

I placed the substance to be tried on a brass plate, which, by means of a piece of thick wire, was fixed horizontally on the knob of the prime conductor, and endeavouring to take the spark from it by means of the ball of the discharger. It was afterwards placed upon a table, or wooden stand, and the shock from a Leyden phial, first passed over it about a quarter or half an inch above its surface, by resting the points of the discharging rods at an inch or more distance from each other, upon the stone to be tried. It need scarcely be observed that in the following experiments it is necessary to close the eyes until the explosion be heard. Some of the results were very beautiful and curious.

New experiments.

All the calcareous species which I tried were more or

 less

Calcareous spars very luminous by the electric shock,

less phosphorescent, and the sparks taken along the surface of a piece of rhombic spar was reflected so intensely as to illuminate the whole table with a brilliant white light.

— particularly chalk.

Amongst the aerated species, common chalk was extremely luminous when the shock was passed at some distance above it, and when passed along its surface the fluid left a very vivid zig-zag track of light, which continued for several minutes.

Ketton stone.

Next to chalk in its phosphoric appearances was a stone called Ketton stone, which consists of large distinct grains, aggluted together like the roes of fish; in passing the shock along its surface, some portion of the stone was shattered, and its luminous grains dispersed in all directions.

Other bodies.

Amongst the several varieties of the sulphate of lime, the specular gypsum, or selenite, is by far the most beautifully phosphoric, but at the same time its light is much more evanescent than in other substances which are less luminous. An oblong six-sided prism of specular selenite shone by the electric explosion with a vivid greenish light, but it continued a very few seconds only. Nitrate of lime fresh made, and tried whilst yet warm, gave small sparks, which upon the surface of the nitrate were quite red, or rather flame-coloured, and it absorbed the electric light but slightly. Muriate of lime was somewhat more phosphorescent than the nitrate. All the fluates absorbed the light freely; the dark purple fluor spars afforded no spark, but allowed the electric fluid to pass in a purple stream, accompanied with a whizzing noise, whilst a yellowish fluor, and another with a greenish tint, which was phosphoric by heat, afforded very good sparks.

Sulphuret of lime particularly luminous.

Sulphuret of lime, commonly called Canton's phosphorus, is much the most luminous by the electric explosion of any substance I have hitherto tried, and affords some beautiful experiments, one of which I cannot help mentioning.

Striking and beautiful experiments.

Mix sulphate of lime with the white of an egg, and spread it about the tenth of an inch thick upon a piece of board; dry it in the air for a day or two (as it dries very slow),

slow), and when perfectly dry it is very hard; place the ends of the discharging rods upon this substance, about two inches asunder, and take the explosion of a Leyden phial. The fluid does not pass over the surface of the sulphuret quietly, but strips it from the board, and disperses it with violence in every direction, giving the appearance of a beautiful shower of fire.

Phosphate of lime. All the bones which I have tried are luminous by the absorption of electric light, and the enamel of the teeth is still more so; ivory is very phosphorescent by the explosion, and readily perforated by it. The shock from a small phial will perforate from nine to twelve ivory fansticks, and the spark renders these thin slips of ivory transparent. Phosphoret of lime gives a very minute red spark, and is but slightly phosphorescent when the explosion is made above its surface. Phosphate of lime.

But its most remarkable property is that of being inflammable by means of a very small shock passed through it. As the flame is readily extinguished, a very small piece of the phosphoret, about half the size of a filbert, may be set on fire several times. —is inflammable by a very small shock.

I have tried many of the testacea and lithophyta, and considering the facility with which all of them imbibed the electric light, I suspect it is a general property belonging to those tribes. The same may be said of all extraneous fossils, which are of a calcareous nature.

Considering how beautifully luminous calcined oyster shells and belemnites are rendered by the electric explosion, I was much surprised to find quick lime fresh from the kiln rank amongst the least phosphorescent of the calcareous genus. Besides the substances here individually specified, I have tried several of the marbles, limestones, stalactites, and spars, all of which were phosphoric. Quick lime is not rendered luminous.

Should any thing curious occur in my trials with the remaining genera, I shall not fail to inform you, if you think this communication worth acceptance.

I remain your's, &c.
WM. SKRIMSHIRE, Jun.

Wisbech, Oct. 16, 1806.

V.

Description of a Portable Blow Pipe for Chemical Experiments. *By* W. H. Wollaston, *M.D. Sec. R.S. &c.*

To Mr. Nicholson.

Sir,

Pocket blow pipe.

The consideration of those instruments which facilitate the attainment of chemical knowledge cannot be thought foreign to the design of a Journal which professes to have for one of its objects the diffusion of chemical information; I am in hopes, therefore, that a short description of a portable pocket blow pipe may be acceptable to many of your readers.

Two tubes slide and draw out, &c.

It consists of three parts, so adapted to each other that they may either be packed together, one within the other, as in Fig. 1. Pl. VIII. which represents them of their actual size, or they may be connected for use, as in Fig. 2. in which the whole is reduced to one half of its real dimensions.

In Fig. 1. the interior tube is shewn to be longer than the exterior; and it is made so, that it may be more readily withdrawn.

In each figure, the upper edge of the large end appears turned outward, in order to diminish the effort of the lips requisite for retaining it in the mouth.

In Fig. 2. it will be seen that the small extremity is placed obliquely (at an angle of about 120°), with the design that the flame impelled by it may be carried to a more convenient distance from the eye, so as to answer the purpose of a longer blow pipe.

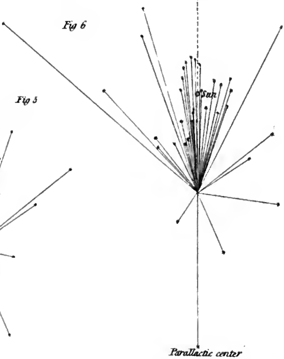
This oblique piece is itself composed of three parts, of which the largest is made stronger than the rest of the blow pipe, that it may not be strained by frequent use. One end of this is closed, and into the other is inserted a small peg of wood, perforated so as to receive the tip, which is intended to be occasionally separated, for the purpose of passing a fine needle into it to remove any accidental obstruction.

The

2 284 29.2

Solar apex

Fig 6



Parallactic center

Sun

Fig 5

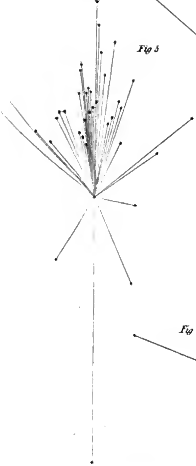
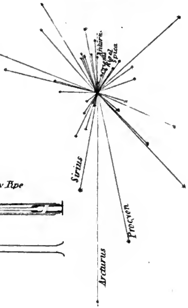


Fig 4



Parrallactic center

Dr Wallastons Blow Pipe

Fig 1

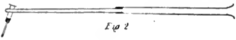


Fig 2

The intention of interposing the piece of wood in this place is, to prevent the communication of heat, which might be inconvenient to the hand that holds the blow pipe.

I remain, Sir,
Your obliged servant,
WM. H. WOLLASTON.

The blow pipe was made by Holtzapfel, Long Acre.

VI.

Experiments on the Growth of White-Thorn. By SAM. TAYLOR, *Esq. of Moston, near Manchester**.

O THE SOCIETY OF ARTS, &c.

Gentlemen,

EVERY one of you, I think, will allow that fences are material objects to be attended to in agriculture; you must also be convinced that there is no plant in this kingdom of which they can so properly be made as the Cratægus Oxyacantha Linnæi, or common White Thorn. In consequence of my being convinced of this, I have been induced to make a few experiments to effect the better propagation of that valuable plant; the result of which, along with specimens of my success, I beg leave to submit to your inspection.

The most valuable fences are made of the white thorn.

In the year 1801, I had occasion to purchase a quantity of thorns, and finding them very dear, I was determined to try some experiments, in order if possible to raise them at a less expense. I tried to propagate them from cuttings of the branches, but with little or no success. I likewise tried if pieces of the root would grow; and I cut from the thorns which I had purchased, about a dozen of such roots as pleased me, and planted them in a border along with those I had bought. To my great astonishment, not one of them died; and in two years they became as good thorns as the average of those I had

Experiments for its propagation.

Cuttings of the branches did not grow; those from the roots did.

* Society of Arts, Vol. XXIII.

Plants of the white thorn successfully propagated from cuttings of the roots.

had purchased. The thorns I purchased were three years old when I got them. In April 1802, I had occasion to move a fence, from which I procured as many roots of thorns as made me upwards of two thousand cuttings, of which I did not lose five in the hundred.

In the spring of 1803, I likewise planted as many cuttings of thorn roots as I could get. In 1804, I did the same; and this year I shall plant many thousands.

I have sent for your inspection specimens of the produce of 1802, 1803, and 1804, raised after my method, with the best I could get of those raised from haws in the common way, which generally lie one year in the ground before they vegetate. They are all exactly one, two, and three years old, from the day they were planted. I was so pleased with my success in raising so valuable an article to the farming interest of this kingdom, at so trifling an expense, (for it is merely that of cutting the roots into lengths and planting them,) that I was determined to make it known to the world, and could think of no better method than communicating it to your Society; and should you so far approve of this method of raising thorns, as to think me entitled to any honorary reward, I shall receive it with gratitude, but shall feel myself amply repaid for any trouble I have been at, should you think it worthy a place in the next volume of your Transactions.

The method of raising the thorns from roots of the plant, is as follows:

I would advise every farmer to purchase a hundred or a thousand thorns, according to the size of his farm, and plant them in his orchard or garden, and when they have attained the thickness of my three-year-old specimens, which is the size I always prefer for planting in fences, let him take them up and prune the roots in the manner I have pruned the specimen sent you, from which he will upon an average get ten or twelve cuttings from each plant, which is as good as thorns of the same thickness; so that you will easily perceive that in three years he will have a succession of plants fit for use, which he may if he pleases increase ten-fold every time he takes them up.

The spring (say in all April) is the best time to plant the

the cuttings, which must be done in rows half a yard asunder, and about four inches from each other in the row; they ought to be about four inches long, and planted with the top one-fourth of an inch out of the ground, and well fastened: otherwise they will not succeed so well.

Plants of the white thorn successfully propagated from cuttings of the roots.

The reason why I prefer spring to autumn for planting the roots, is, that were they to be planted in autumn, they would not have got sufficient hold of the ground before the frost set in, which would raise them all from the ground, and, if not entirely destroy the plants, would oblige the farmer to plant them afresh.

I have attached the produce of my three-year-old specimen to the plants it came from, cut in the way I always practise; on the thick end of the root I make two, and on the other end one cut, by which means the proper end to be planted uppermost, which is the thick one, may easily be known.

Although I recommend the roots to be planted in April, yet the farmer may, where he pleases, take up the thorns he may want, and put the roots he has pruned off into sand or mould, where they will keep until he has leisure to cut them into proper lengths for planting; he will likewise keep them in the same way, until planted.

The great advantage of my plan is: first, that in case any one has raised from haws, a thorn with remarkably large prickles, of vigorous growth, or possessing any other qualification requisite to make a good fence, he may propagate it far better and sooner, from roots, than any other way. Secondly, in three years he may raise from roots a better plant, than can in six years be raised from haws, and with double the quantity of roots; my three-year-old specimen would have been half as big again, had I not been obliged to move all my cuttings the second year after they were planted.

It would not be a bad way, in order to get roots, to plant a hedge in any convenient place, and on each side trench the ground two yards wide, and two grafts deep; from which, every two or three years, a large quantity of roots might be obtained, by trenching the ground over again, and cutting away what roots were found, which

Planks of the white thorn successfully propagated from cuttings of the roots.

which would all be young and of a proper thickness. I do not like them of a larger size than the specimens sent.

I am at present engaged in several experiments, to endeavour to propagate the thorn from the branches, which, if successful, I will communicate to you; but I am of opinion, that what is now done is sufficient.

Should the Society require any further explanations, I shall be happy in doing my utmost to furnish such explanations*.

SAMUEL TAYLOR.

Moston, near Manchester,
May 6th, 1805.

VII.

On the Phantasms produced by Disordered Sensation. In a Letter from a Correspondent.

To Mr. NICHOLSON.

SIR,

Spectres seen by Nicolai from nervous indisposition.

I HAVE just been perusing in your Journal for Nov. 1803, the Memoir of Nicolai, on spectres which haunted that intelligent and estimable man for two months. His narrative developes many curious topics of investigation, respecting our perceptions, and may perhaps lead to a farther explanation of the laws by which our trains of ideas are governed, and the mechanism of our organs of thought; concerning which, so little is known. The perusal induces me to send you a few more facts relative to the same obscure subject.

These phenomena are by no means uncommon.

Many persons, particularly females, within the circle of my personal knowledge, have related to me incidents of the same nature, arising from nervous indisposition. Nothing is more common than the appearance of figures and sounds in fevers; and they are very frequently exhibited to persons in apparent good health. In all the instances related to me, the parties were aware of the

* Specimens were sent to the Society.

objects

objects being the mere consequences of indisposition, or what may be called internal sensation, and spoke of them as such. It is not a month since I was sitting alone with a lady, for whose powers of mind and moral habits, I have the highest respect, when after a short and sudden pause, she said—"This moment I saw M——— standing in "his usual manner just behind your chair, and a little "while afterwards he was in the corner of the room:" Upon my inquiring respecting the appearance, she said the figure was paler, or less clearly visible, than usual, and that it gradually faded away.

Account of an instance.

I know a gentleman, at present in the vigor of life, who in my opinion is not exceeded by any one, in acquired knowledge, and originality of deep research; and who, for nine months in succession, was always visited by a figure of the same man, threatening to destroy him, at the time of going to rest. It appeared upon his lying down, and instantly disappeared when he resumed the erect posture. This was not related to me by himself, but by another friend, and his absence has since prevented my inquiring farther.

Phantasm which appeared for many months on lying down to rest.

Little doubt remains in my mind, that many of the stories of apparitions, which have been in all ages so generally received, were true, though probably incorrect, from the influence of the imagination under an impression of terror. When I was a boy, I once or twice in the night awoke with the disease commonly called the night-mare; and then the fit was accompanied with a sense of weight, as if caused by a person actually pressing on me, and touching me with cold hands; and in the momentary interval between one crisis and the next, I had a consciousness that that person hurried round the room and came back to torment me again, before I could recover my speech or motion. But afterwards, when I was older and considered these as the effects of disease, I had an attack, in which I experienced no terror, nor had any concomitant notion of an external agent; and as soon as I felt a remission of the rigor, I sprung up and was relieved; no other consequence remaining but a slight tremor of the surface of the body.

Many of the stories of apparitions have been true; but originated in disease, assisted by fancy.

Incubus, or Night-mare.

About twelve years ago, I had an attack of fever, arising

Narrative of the phantasms

produced by disordered sensation in fever.

sing from some deep seated inflammation which caused acute pain in the left side. It was occasioned by a cold caught at the breaking up of the hard frost in the Spring of 1795. The pulse was generally about 110 in the minute, and the illness, which lasted some weeks, was accompanied with disordered perception, through almost its whole duration. My recollections of what then happened, renewed by occasional meditation on the subjects since that time, are now so far impaired, that some of the particulars recur in a less striking manner; the exact order of their succession, and time of their respective duration, are less certain than these would have been, if my first intention of writing down the various phenomena soon after the event, had been carried into effect.

The disorders of perception form a subject of interest, where a law of action can be indicated.

The phantasms or delusions which accompany and mark disordered sensation, (which term I would use in contradistinction to disorder in the powers of memory reasoning or the moral habits) are perhaps too frequent and too little varied, to afford much interest in describing them, unless where the narrative can point out some law which the effects may seem to follow, or may afford some general inferences that may prove valuable as rules of conduct under such sufferings. It must no doubt be a considerable advantage and consolation to those who might ascribe these visions to supernatural powers, or who might be driven to insanity by impatience or terror, on the supposition of reality, for want of knowing these phenomena of disease;—it must, no doubt be highly beneficial that they should have such knowledge: but the events I offer to your readers, are, in my opinion, principally remarkable for a certain connection they shewed with that common law o. association, by which our usual train of ideas is so immediately and rapidly governed.

Patients may derive much advantage from being aware of the phenomena.

Commencement. Slight but not permanent defect of memory.

At the commencement of the fever, a slight defect of memory was perceived in forming the phrases for dictating a letter; but this did not last, and I found no difficulty afterwards in performing arithmetical and other processes by memory to as great an extent as my usual habits could have gone. The first night was attended with great anxiety, and the fatiguing and perpetual recurrence of the same dream. I supposed myself to be in the

the midst of an immense system of mechanical combination, all the parts of which were revolving with extreme rapidity and noise, and at the same time I was impressed with a conviction that the aim or purpose of this distracting operation was to cure my disorder. When the agitation was carried to a certain height, I suddenly awoke, and soon afterwards fell again into a doze, with repetition of the same dream. After many such repetitions it occurred to me that if I could destroy the impression or conviction, there might be a probability that the delirious dream would change its form; and as the most likely method, I thought that by connecting some simple visible object in my mind, with the notion of cure, that object might be made to occupy the situation of the rapidly moving objects in the dream. The consequence, in some measure, answered my expectation; for upon the next access, the recollection of the figure of a bottle, to which I had previously directed my mind, presented itself, the rotation ceased, and my subsequent dreams, though disturbed, were more various and less irritating.

Anxiety: incessant recurrence of the same dream.

This recurrence was prevented by an act of discipline in the mind.

The medical treatment consisted in the external application of leeches to the side, with venesection, and the saline mixture was taken internally.

Medical treatment.

A second night was passed with much agitation in repeated dozing, with dreams, in which, except with regard to the strangeness and inconsistency of the objects that offered themselves, it was difficult to distinguish the time of sleep from that of wakefulness. None of that anxiety of mind remained which had added to the sufferings of the preceding night. When morning came, the state of the sensations had either undergone a change; or it was more easy, as Hartley* remarks, for the real impressions of surrounding objects, to predominate over the phantasms of disease. Being perfectly awake, in full possession of memory, reason and calmness, conversing with those around me, and seeing, without difficulty or impediment, every surrounding object, I was entertained and delighted with a succession of faces, over which I had no control, either as to their appearance, continuance or removal.

Disturbed night:—

—and in the morning, a succession of phantasms of faces succeeding each other for a long time.

* On Man.

Manner of their appearance, duration and change.

They appeared directly before me, one at a time, very suddenly, yet not so much so, but that a second of time might be employed in the emergence of each, as if through a cloud or mist, to its perfect clearness. In this state each face continued five or six seconds, and then vanished, by becoming gradually fainter during about two seconds, till nothing was left but a dark opake mist, in which almost immediately afterwards appeared another face. All these faces were in the highest degree interesting to me, for beauty of form and the variety of expression they manifested of every great and amiable emotion of the human mind. Though their attention was invariably directed to me, and none of them seemed to speak, yet I seemed to read the very soul which gave animation to their lovely and intelligent countenances: admiration and a sentiment of joy and affection when each face appeared, and regret upon its disappearance, kept my mind constantly rivetted to the visions before it; and this state was interrupted only when an intercourse with the persons in the room was proposed or urged.

Theory of Hartley; that the visions of fever are common ideas or thoughts exalted by irritability.

It was in my recollection that Hartley in his work upon Man adopts a theory, that the visions of fever are common ideas of the memory recalled in a system so irritated, that they act nearly with the same force as the objects of immediate sensation, for which they are accordingly mistaken: and therefore it is, says he, that when delirium first begins, if in the dark, the effect may be suspended by bringing in a candle, which by illumination gives the due preponderance to the objects of sense.

It is not the fact.

This, however, I saw was manifestly unfounded. It was in my power to think of absent objects (e. g. of sight) as usual, but they did not appear. The ideas were in the mind as usual, and at the very same time, the real objects of sense, and the objects of diseased sensation stood visible before me.

Ideas, sensations and phantasms can be all distinctly present at once.

When my attention was strongly fixed on the idea of an absent place or thing, the objects of sensation and of delirium were less perceived or regarded. When the mind was left in a passive or indolent state, the objects of delirium were most vivid, and the objects of sensation, or real objects in the room, could not be seen. But

But when by a sort of exertion, the attention was roused, the phantasms became as it were transparent, and the objects of sensation were seen as if through them. There was not the least difficulty in rendering either object visible at pleasure; for the phantasms would nearly disappear, while the attention was steadily fixed on the real objects. Each particular phantasm was neither hastened nor retarded in its whole appearance or duration by this process.

The voluntary attention of the mind, gives vigour and strength to either of the three, at pleasure.

After a morning passed in this manner, I had a visit from Dr. C——, to whom I related the effects, and among other remarks I observed that I then enjoyed the satisfaction of having cultivated my moral habits, and particularly in having always endeavoured to avoid being the slave of fear. "I think," said I, "that this "is the breaking up of the system, and that it is "now in progress to speedy destruction. In this state, "when the senses have become confused, and no longer "tell me the truth, they still present me with pleasing "fictions, and, my sufferings are mitigated by that calmness which allows me to find amusement in what are "probably the concluding scenes of life."

Advantages of self-command, conversed upon.

I give these self-congratulations without scruple, because I am an anonymous writer, and more particularly because they led to an observation of fact, which deserves notice. When the doctor left me, my relaxed attention returned to the phantasms, and some time afterwards, instead of a pleasing face, a visage of extreme rage appeared, which presented a gun at me and made me start; but it remained the usual time and then gradually faded away.

The phantasms are affected by the conversation

This immediately shewed me the probability of some connection between my thoughts and these images; for I ascribed the angry phantasm to the general reflection I had formed in conversation with Dr. C———. I recollected some disquisitions of Locke in his Treatise on the Conduct of the Mind, where he endeavours to account for the appearance of faces to persons of nervous habits. It seemed to me, as if faces, in all their modifications, being so associated with our recollections of the affections or passions, would be most likely to offer themselves in delirium: but I now thought it probable that other objects would

And this suggested a connection between thoughts and the visionary appearances.

Attempt to

alter the appearance of the phantasms, would be seen if previously meditated upon. With this motive it was that I reflected upon landscapes and scenes of architectural grandeur, while the faces were flashing before me; and after a certain considerable interval of time, of which I can form no precise judgment, a rural scene of hills, vallies and fields appeared before me, which

—which succeeded. was succeeded by another and another in ceaseless succession; the manner and times of their respective appearance, duration and vanishing being not sensibly different from those of the faces. All the scenes were calm and still, without any strong lights or glare, and delightfully calculated to inspire notions of retirement, peace,

They disappear at the instant of taking a medicine:— tranquillity and happy meditation. I do not remember how long these lasted, but think it was the next morning that they all vanished, at the very instant of taking a draught, composed of lemon juice, saturated with potash, with a small addition of the pulvis londinensis. I cannot think the effect was owing to any peculiar virtue of this medicine (for it took place before the draught had actually entered the stomach) but merely to the stimulus of the subacid cold fluid.

—but return again, though in other forms. How long the appearances were suspended, I did not note, or have now forgotten. The fever continued with the same frequency of pulse, and pain in the side, attended with yawning and great increase of suffering while in the prone posture. Notwithstanding the saline antimonial medicine was continued, the figures returned; but they now consisted of books, or parchments, or papers containing printed matter. I do not know whether I read any of them, but am at present inclined to think they were either not distinctly legible or did not remain a sufficient time

—which were changed by volition. before they vanished. I was now so well aware of the connection of thought with these appearances, that by fixing my mind on the consideration of manuscript instead of the printed type, the papers appeared, after a time, only with manuscript writing; and afterwards by the same process, instead of being erect, they were all inverted or appeared upside down.

Delusions of the sense of hearing. It occurred to me that all these delusions were of one sense only; namely, the sight; and upon considering the recurrence of sounds, a few simple musical tones were afterwards

afterwards heard, for one time only; soon after which, having dropped asleep, an animal seemed to jump upon my back, with the most shrill and piercing screams, which were too intolerable for the continuance of sleep.

Another mode of delusive perception of visible objects.

Diseased perceptions of the hearing did not again recur, and I do not remember by what gradation it was, that the frequently changing appearances, before the sight, gave place to another mode of delusive perception, which lasted for several days. All the irregularly figured objects, such as the curtains or clothes, were so far transformed that they seemed to afford outlines of figures, of faces, animals, flowers and other objects, perfectly motionless, somewhat in the manner of what fancy, if indulged, may form in the clouds or in the cavity of a fire; but much more complete and perfect, and not to be altered by steady observation or examination. They seemed to be, severally, as perfect as the rest of the objects with which they were combined, and agreed with them in colour and other respects.

I can make so few inferences or observations upon the several other characters, which these diseased sensations assured, that I shall not attempt to describe them.

Various accounts of ocular facts.

Various authors have given narratives which coincide with the preceding, in part; and as analogy is the great clue for investigating the phenomena of nature, I will give a few facts and remarks which may bring us more to a point.

Phantoms of real objects produced by emotion of mind.

None of the phantoms in my illness were of known places, objects or persons. But on another occasion, when I accidentally fell into the sea, and after swimming a certain time without assistance, began to despair of my situation; the image of my dwelling and the accustomed objects appeared with a degree of vividness, little different from that of actual vision. Mr. Sturt, M. P. when greatly in danger some years ago, by being wrecked in a boat, on the Edystone rocks, relates, in an account which appeared in the papers, that his family appeared to him in this extremity. "He thought he saw them." I think both these instances are referable to Hartley's Theory. The illusions of figures appearing to persons near death are very common.

Sleep

Delirium precedes sleep. Sleep is, I think invariably preceded by a diminished power of judgment and the appearance of phantasms.

Dreams are like the phantasms of fever. The objects of dreams appear to be of the same class, or description, as those I had in fever. Like them they appear uncontroled by the will for the moment, and resemble the objects for sense; and like them they can be often traced to some preceding thought or incident. Is not a certain degree of debility one of the conditions required for the appearance of these phantasms?

Comparison of the visible and audible perceptions caused by disease. The ear is much more an instrument of terror than the eye. Diseased perceptions of sight are more common than those of hearing, and they are in general borne with more tranquillity. A few simple sounds usually constitute the amount of what the ear unfaithfully presents; but when incessant half-articulated whispers, sudden calls, threats, obscure murmurs, and distant tollings are heard, the mind is less disposed to patience and calm philosophy. Instances however are not wanting, in which musical combinations of enchanting melody haunt the mind, and occupy the senses of those who are oppressed with indisposition.

I will not make this letter longer by apology. Do with it what you please, and I shall continue,

A grateful sharer
In your labours.

L. M.

VIII.

Early Account of the Bath Waters, by JOHN MAYOW. *Extracted in a Letter from* W. R. CLANNY, *M.D. Hon. Member of the Royal Philos. Soc. at Edinburgh, Senior Physician to the Durham Infirmary.*

To MR. NICHOLSON.

BEING lately much engaged in the interesting study of mineral waters, amongst other books on the subject I read with much pleasure a scarce work, entitled, "A Discourse of Bathe, and the Hot Waters there, &c. by Tho. Guidott, M. B. London, 1676." The author has inserted

inserted a translation of Dr. Mayow's Analysis of the Bath Waters, for the purpose of refuting it; and in his zeal to support his own analysis, in which he asserted that the Bath Waters contained nitre and sulphur, he has preserved Dr. Mayow's analysis, which, even at this enlightened period of chemical science, must be esteemed as a superior production. On reading it I was impressed with the idea that it would be interesting to many of your scientific readers, who may not have seen the work I allude to.

Early examination of the Bath waters.

How far Dr. Mayow understood the oxydation of metals his own words will shew; nor will any unprejudiced reader suppose that the thread of science was broken in his hand.

Dr. Mayow's Analysis was published in the Latin language, which I have not seen; but it is rather an advantage to general readers to have a translation in the vernacular language, and being translated by a cotemporary, though a rival Physician, may in some degree assist in fixing our ideas of the meaning of the author.

Since writing the above, Dr. Gebbes' excellent analysis of the Bath Waters has come to hand; his judicious remarks on Dr. Mayow's Analysis preclude me from offering any further observations, except, that by Dr. Mayow's Analysis I was led to understand that the Bath Waters contained iron in a pure state, which is since corroborated by Dr. Gibbes' late communication to your excellent Journal. I shall now give the whole chapter from Mr. Guidott's work, wherein is inserted Dr. Mayow's Analysis.

CHAP. II.

" *The Opinion of a late Author concerning the Nature of the Baths of Bathe.*

Joh. Mayow, L.L.D. & M.D.

" And here I cannot but take notice of a novel writer, who magesterially thus determines: *Quod ad nitrum et sulphur attinet, quibus Thermas Bathonienses imbutas esse hactenus creditum est, eorum neutrum aquis thermarum istarum solutum esse arbitror: As to what concerns nitre and sulphur with which the Baths of Bathe have*

Early examination of the Bath waters.

hitherto been thought to be impregnated, I suppose there is nothing of either of them dissolved in the waters.

" A bold assertion! which had it been vented and believed but fifty years ago, would have prevented much trouble in evincing the contrary; but since 'tis the fashion to be peremptory, I do assert, *That both nitre and sulphur are to be found in all the baths of Bathe, and that dissolved in, and mixed with, the body of the waters.* In order to the proof of which I shall take some account of the forementioned author's XVth Chapter of his tract of Sal-Nitre, the arguments he hath against it, and his opinion to the contrary.

" His words therefore, as well as I can translate them, are these :

" Among the most celebrated bathes, we may justly reckon those of *Bathe*, in which admirable waters a continual vestal and sacred fire is maintained, as if things of a most different nature were interleagued. Before I come to the manner how these bathes receive their heat, it will not be improper if I make some inquiry into the contents of these waters.

" It is therefore manifest, that the bathes of Bathe are impregnated with a certain salt of an acrid nature; for if any sal alkali, or volatile salt purely salined, be mixed with these waters, a precipitation will ensue, and the waters will become turbid, and of a milky nature.

" Moreover, the Bathe waters powred on boyling milk, will coagulate it, as any other acid doth.

" Neither doth this acid salt seem to be the only salt of the Bathe, but is complicated with an alkali; for if the water be evaporated quite away, a certain salt of a more fixed nature will be found in the bottom of the vessel, which, on the powring of any acid on it, will ferment.

" Of the same nature also are the mud and sand of the bathe, which are wrought up with the springs; for any acid liquor being powred on them, an ebullition will follow.

" There may be also observed in these waters a salt, or rather a lime-chalk kind of earth, sticking to the bottom of the gouts, or passages, almost in all places where the water passeth.

From

"From what hath been said may be collected, That the bathes of Bathe are impregnated with a certain acid saline salt; and the salt of the bathe seems not much unlike tartar vitriolated, or aluminous salt.

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"The reason why these salts destroy not one another, but each of them ferments with its contrary salt, may be understood from what hath been delivered in the former Chapter; to wit, these salts are so imperfect, that in conjunction they cannot destroy one another. But more of these salts hereafter.

"As to nitre and sulphur, with which the Bathes have hitherto been thought to participate, I suppose that neither of them is dissolved in those waters.

"That there is no nitre in the waters appears by this. That the salts that remain after the evaporation of the Bathe Water, put on a coal, burn not as nitre doth. Although I shall not deny, that those immature salts of an alkali nature (which are also contained in the sand and mud of the Bathe) being exposed some time to the air, may, perhaps, by its influence, be converted into nitre.

"As to sulphur, which hath been so much reported to be in all bathes, 'tis not, I believe, dissolved in these waters. Because,

"If a solution of alom, vitriol, or any other salt, whether acid, or fixt, be mixed with the water of the Bathe, sulphur discovers not itself to be precipitated, either by a fetid smell, or any other sign, which notwithstanding in the solution of sulphur in the water of unslack'd lime, or made into lixivium, doth appear, when the sulphur by the effusion of any acid liquor is precipitated.

"I am not ignorant that the water of these bathes, if salt of tartar, or a purely volatile salt, be cast into it, will presently turn white, as is declared before, which colour proceeds not from sulphur, but a stony, or aluminous matter precipitated, not much unlike to what is observed in the water of unslack'd lime, when any fixt salt is mixed therewith; in which, notwithstanding, it is not to be supposed the sulphur is dissolved; for if sulphur be boyled in water of unslack'd lime, the water becomes

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white, not by the effusion of a fixt salt, as before, but of an acid; so that the fixt salts may dissolve sulphur, but not precipitate it. Wherefore if sulphur be contained in the waters of the Bathe they would be precipitated, not by a purely saline, as formerly, but an acid salt, and the sulphur so precipitated would discover itself by a fetid smell, which it doth not do.

" To which I add, that an acid salt, or something aluminous, doth seem to predominate in the bathes aforesaid, so that they become altogether unfit to dissolve the sulphur.

" Moreover, if common sulphur be boyled in those waters they are never tinged with a yellow or sulphurous colour, neither can sulphur by any means be precipitated from the decoction, as I have often experimented.

" And therefore I must admire the famous Willis, in his Treatise of the Heat of the Blood, should affirm, that sulphur boyled in Bathe Water may be dissolved after the same manner, as if boyled in water of unslack'd lime.

" Now if sulphur seems to be dissolved in the waters aforesaid, the occasion of the mistake, I suppose to be, that the decoction was made in a vessel, in which some fixt salt had been decocted, so that the solution of the sulphur may be made by some particle of a fixt salt, with which the vessel might be seasoned.

" Concerning the bathes of Bathe, 'tis the common opinion that silver dipped into them is colored yellow, in the same manner as if it were cast into a solution of sulphur, and hence it is supposed that the Bathes have sulphur in them; but experience evinceth the contrary; for silver put into the Bathe Water becomes not reddish, or yellow, but rather black.

" The mistake may seem to arise from this, that 'tis customary with the Bathe-Guids to tinge and as it were guild over pieces of silver with the salino-sulphurous mud, or dung, such as is often found in houses of office, and put them off to strangers, for a little profit, as if they were coloured with the Bathe-water.

" And here this is to be noted, that a kind of bituminous mud, with a small pittance of common sulphur, is brought

brought up with the springs, which only swims on the top, or else continues at the bottom, but never is dissolved in the waters themselves. Early examination of the Bath waters.

"Neither is sal armoniack, as some imagine, to be found in these waters, for if on the solution of sal armoniack, salt of tartar be injected, the purely saline volatile salt (of which sal armoniack in part doth consist) being at liberty from the acid salt, to which it was formerly united, will presently fly off into the air, and will quickly be discovered by a pungent affecting the nostrils, which is never observed in the Bathe-waters.

"Lastly, as to vitriol, the Crosse and Hot Bathe seem to have none at all; for if galls are beaten and infused into these waters they neither turn purple nor black, which would certainly be, if these waters had vitriol in them.

"The King's Bathe seems to have a little vitriol in it; for if some beaten galls are cast into that water, it will have a light tincture of a black purple colour.

"'Tis also to be noted, that a certain mineral of a metallick nature ariseth out of the earth, with springs of the Bathe, which is easily turn'd into vitriol. For if any acid liquor be affused on the sand, (which breaking out with the springs, is found in the bottom of the Bathe,) it being corroded with an acid menstruum, not without a remarkable effervescence, will in part be converted into vitriol, just as it happens to the filings of iron corroded with an acid liquor.

"For if that sand of the Bathe corroded with an acid liquor, be put into the infusion of galls, the liquor acquires an atro-purpureous colour. Whereas if the infusion of galls be put on the sand newly taken out of the bathe, and not corroded with an acid liquor, it will, by no means, be of a purple colour; an apparent sign, that the metallick sand of the Bathe, unless corroded with an acid menstruum, doth not turn to vitriol.

"It is further observable, that the sand of the bathe kept some time, and exposed to the open air, will of its own accord be converted into vitriol; for if that sand be mixed with the infusion of galls, the water will contract an atro-purpureous appearance.

"Moreover,

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"Moreover, if it be laid on the tongue, it hath a perfect vitriolick taste; and no wonder, for the nitro-aereous spirit, after some time, closeth with the metallick mineral, and salino-sulphureous marchasite, of which vitriol useth to be made, mixed in the sand, and causeth it to ferment, and at last, as was shewed before, converts it into vitriol."

Should the above extract prove interesting or amusing to your scientific readers, I shall be much gratified.

I am,
With much respect,
Sir,
Your most obedient servant,
W. R. CLANNY.

Durham, 15*th Nov.* 1806.

IX.

The Improvement of Boggy Land by Irrigation, as carried into Effect. By Mr. William Smith*.

Sir,

Improvement of boggy lands by irrigation.

HAVING paid very considerable attention to improvements of land by irrigation, and applied the water in a manner which I believe to be new and advantageous; I beg leave to submit my account thereof to the Society of Arts, &c. Upon this plan, the rushes and noxious parts are destroyed, the land is rendered firm, and grasses of good quality spring naturally. Even ferruginous waters will have a good effect thus used.

I am, Sir,
Your obedient servant,
William Smith.

Buckingham-Street,
Oct. 31, 1804.
To Charles Taylor, Esq.

* Soc. of Arts, Vol. XXIII. The silver medal was awarded for this communication.

Mode

Mode of improving Boggy Land.

Improvement of boggy lands, by irrigation.

The whole surface of the boggy ground was pared with a breast-plough, and the peaty matter thrown together in ridges, like common high-ploughed land, with a ridge, like a head-ridge, at one end of each set of ridges. Each ridge has a cut or channel for water on the top, and a drain in the furrow or hollow between it and the next ridge. The head-ridge has a larger channel for water on its top, which supplies all the other ridges with water, and this main ridge is itself supplied by its connexion with a larger channel or feeder, which first conveys the water out of the common brook-course into the meadow.

The furrow between each head-ridge and the ends of the beds has a larger drain, into which all the channels of drains in the furrows discharge their water, and which is, by this main drain carried into the brook-course again. Thus the water is diverted out of its usual channel, only to float over the surface of the land, and run into that channel again lower down.

To get the water high enough to swim over the surface of any piece of ground, it is generally necessary to make a dam in the original channel, to pen up the water till it rises to the surface, or near it, and convey it along a channel which shall have less fall than the brook, until it can be got out upon the surface. The length of such conduit or drain must therefore depend upon the fall in the lands which lie parallel to the original channel of the water; and the quantity of land that can be covered with water, depends upon the distance between the proposed new channel and the old ones.

And, to perform this business in the most methodical manner, it is necessary to new model the surface, otherwise the water (which will always find its level) would lie too deep, or move too slowly over the low places in the ground, and thereby injure the grasses by a redundancy of water, while all the higher parts of the ground would appear like little islands above the surface of the water; and consequently receive no benefit from such an imperfect system of irrigation.

Where these inequalities of surface are large and numerous,

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rous, it will be attended with much more expense to make such land marks into a regular form for floating, on account of the great expense of wheeling the earth from the hills to the hollows. In these cases, it is necessary (in order to avoid expense) to adopt an irregular method of floating: by taking advantage of such irregularities of surface, a meadow may often be floated at a quarter of the expense required to put it into a regular form, and this method is found to answer the purpose very well, if the works are properly laid out, with the spirit level. When the fall of water is ascertained, the form of the ground is the next thing to be attended to; if there are no natural declivities in the surface, down which the water may run from the overflowings of a cut on the summit into a drain in the hollows, so that the water may keep constantly running down such slopes by a regular current, which prevents a diminution on the ridges and a quick discharge in the lower drain; to avoid an accumulation in the furrows, it must be made with good slopes and plenty of drains; these, with a constant supply of water in the winter, are the most essential parts of a water meadow. The water must be constantly kept moving over the surface, and the practice proves, that where the water moves the quickest, there is always the most grass.

And, as the water must be constantly running off the land, it follows that it must be constantly running on, to keep every part of the surface properly supplied; and this requires a much greater quantity of water than is commonly imagined by those, who are wholly unacquainted with the practice of irrigation. In fact, every good water-meadow should be formed so that it may be said to be nothing but a wide extended channel for the water, no part of which should be too deep to prevent the points of the grass from appearing above its surface, consequently the water cannot be seen when the grass begins to grow. Yet it will still find its way between the shoots, and nourish the grass without bearing it down, or excluding it from the benefit of the air and sun: this is a state, in which the grasses of a water-meadow increase very rapidly; in this state, no water can be seen in any part

part of a meadow, but in the cuts which bring it on and drains that take it off; the motion down the slopes is only perceptible where it runs off the upper cut and in the lower drain; in the still more perfect parts, when the grass has got a considerable shoot, even this part of its motion is not perceptible; and a well-regulated meadow, in the spring, cannot be known to be in a state of irrigation without walking into it. The water running among its shoots, soon becomes perceptible to the foot which proves that there is no inconsiderable quantity running down the slopes, though its motion upon that part cannot be seen.

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It is therefore one of the fundamental principles of irrigation, to keep the water moving, and that in such well regulated quantities as shall neither be too great nor too little; for both of the extremes are alike unfriendly to vegetation; but I believe there is much more mischief done to a water-meadow by giving it too little, than too much water, and the greater the supply the less nicety is required in the adjustment, if the meadow is so laid out as to prevent its accumulation in any part thereof. But where the quantity of water is small, it is necessary to be very nice in the distribution of it, in order to receive the full benefit of the stream upon as much land as it is capable of floating.

Here again we must not run into extremes, and try to get the water over too much land at a time, and thereby prevent the grass from receiving the full benefit of a quantity of water which is capable of giving it a good soaking: what that quantity is, will be best determined by practice, for some ground requires much more water than others.

In case of a short supply of water, which is extended to the improvement of as much land as it is capable of covering, according to the best principles of irrigation, it will be better to unite all the water upon such a portion of the work as practice shall prove it capable of covering well, and to let that part have the full benefit of the water as long at a time as is necessary to give a good soaking, or as long as it may be kept off the other parts without injury.

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In some meadows, after they have had a complete soaking, which has saturated the soil, and the grass has thickened upon the surface, vegetation will not be retarded for some time for want of water, and those parts which were forced the most in the autumn, will require the least in the spring. It will therefore be always advisable on account of the water and a succession of grass, to get some part of the water-meadow as forward as possible in the autumn, that that part may be dried and fed the first in the spring, while all the water is employed in forcing on those other parts of the meadow which were neglected in the autumn.

By a prudent management of the water in uniting its vegetative powers upon those pieces of meadow which are disposed to produce the earliest vegetation in the spring, and so on in succession, from the earliest to the latest pieces of ground, or those which can be made so; a regular succession of grasses might be obtained, which would be much better than trying to get the whole alike, especially, if the quantity in one person's possession be very considerable, and his quantity of water likely to fail for barely to suffice for the purpose.

This method of using the water in succession upon portions of the meadow, which practice shall prove it capable of covering at one time, will be applicable to most meadows; for there are few, that are well formed, that have too much water, especially in the winter, or where there are any mills or navigations; I have generally observed that the best meadows upon the large streams, are those which have the most water and the best falls.

Account of the Nine Acres of Water Meadow, on Prisley Farm near Fletwick, Westoning, and Tingrith in Bedfordshire.*

As the quantity of water is sometimes insufficient to float the whole of this meadow at once, it has been contrived

* A map of this meadow, but without any account of the method of forming it, may be seen in the Communications of the Board of Agriculture, Vol. IV. page 341.

trived to be divided into three parts, by means of two large hatches within the meadow. Each of these principal divisions may again be divided into still smaller parts, by putting a common hatch or board made to the shape of either of the main feeders, which will stop the water out of any part, and force so much the more upon that which is intended to be floated. These contrivances are often necessary on account of the great scarcity of water, and also for the purpose of employing all the water upon any one part of the meadow, while the grass is feeding off the other; and (if the levels will admit of it) something like this ought to be done in every good water-meadow, for it is not merely the elevated or high-ridged form of the surface, which constitutes a good water-meadow, but such a disposition of the parts as is best calculated for the general purposes to which the land, the water, or its produce, may be most advantageously applied. The three parts of this meadow are upon two different levels, so that the drawing of either of the hatches before mentioned lays all the high part dry, and puts either the North or the South part of the lower level afloat at the same time. By keeping down one of those hatches and opening the other, all the water may be turned either upon the North or the South part of the low level, as occasion may require; or if both the hatches be shut down, the whole of the water may be used upon the high level, or two first sets of beds.

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If there is more water than is sufficient to float either of the three parts separately, either of the two regulating hatches may be fixed at such a height as to use the remainder on the upper level; or the high level of the meadow may be made to receive its full quantity of water, and an opening be left under one or both of the hatches, so as to distribute the remainder of the water on either of the parts of the lower level, wherever it may be wanting; or the whole of the water may be used upon one of the lower levels, by adjusting the hatch so that that part shall have sufficient water, and drawing up the other high enough to discharge the surplus; or, if one part is floating, and neither of the other pieces in want of water, any overplus may be turned down the waste ditch which di-

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vides the meadow from the upland, by drawing the outside or main hatch, high enough to discharge such surplus water under it.

The water is capable of all these variations, but there will seldom be any occasion for turning any water to waste, as it may generally be all employed upon the meadow or upon a third of it. If the other two parts should be in use, it will be found most advisable to feed only one part of such meadow at a time, as the other two-thirds might then be floating alternately.

When that third has been fed off, the most forward of the other two may then be laid dry for feeding, and the new-fed part floated in its stead. By this plan of feeding one-third at a time, and keeping the other two-thirds afloat at the same time, either together or separately, according to the quantity of water, the water will be always constantly employed from the first commencement of floating to the conclusion of the feeding and floating after it; when the whole may be shut up together for mowing.

The spring floating may be continued at intervals, (if the water be not foul) till the grass has gained a considerable height, but it must only be put on for a day or two at a time to cool the ground, and keep the grass growing. This management, if it be well conducted, will be of great service in forwarding the crop and increasing the bulk; the ground will also be the cooler and better for it when the crop comes off, consequently, it will occasion the after-grass to grow so much the quicker. No time should be lost in putting on the water immediately after the hay has been removed; or, as soon as one-third of the meadow can be cleared, the water should be immediately put upon that part till it is pretty well soaked, and then upon the other parts, in their turns, as soon as they are cleared. Great care should be taken both in feeding and taking off the hay, that it be done with a view of clearing that part first, where the water can be first applied to the purpose of producing another crop. The water should never run to waste but in the height of summer, when the grass may be high enough to form a thick cover to the ground, and keep it cool and moist enough

for

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for the purposes of vegetation without the aid of water; and also at the end of summer or autumn, when, if the meadows are fed with sheep, there may be some danger of rotting them by using the water at this time of the year. It will appear to those who are acquainted with the management of Wiltshire water-meadows (by the account annexed, which I received from his Grace the Duke of Bedford, and which states the quantity of grass cut and the time of feeding the meadow), that the grass was begun to be fed off before it war fit; and, from the long time that the sheep were kept upon the ground during the months of February, March, and April, there was much of the water wasted, which should have hourly been employed at that most prolific season. Experience proves, that there is no danger of getting the grasses too strong upon the ground at this early season, and that crops which are six or seven inches high, and apparently too coarse and high for a bullock to feed, are eaten with the most eagerness by sheep in the spring; and those parts where the grass is the thickest and most luxuriant, are always fed the closest, and sought after with the greatest avidity. This being contrary to the common habits of all animals which graze upon dry pastures, where they give a decided preference to short and sweet herbage, may lead many persons to think that the grass of a water meadow may be too high and luxuriant for sheep; but experience has proved, that such long grass is neither unfriendly nor unsavoury to them; and we know, that the grass always grows the fastest when it has gained considerable height and strength. It will also thicken at the bottom, and the roots will get much stronger hold in the ground, and consequently will not be subject to feel the want of water so soon during the time of feeding, and be able to make a much stronger shoot as soon as it is shut up again, and the water restored to it. The greatest crop will also be of the best quality both in grass and hay, and will always be fed much closer and evener than in those places where the floating has been any ways deficient. The drowner, as he is generally called, or the man who has the superintendance of water-meadows, should therefore endeavour to make every part of the crop as uniform as possible;

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possible; for no meadow can be said to be complete till that is accomplished, and a good eye may easily discover the management of a meadow by the crop upon the ground, whether it be in the spring, summer, or autumn: for, if the grass appear patchy, or of different sorts and colours, there can be no doubt but that the water has been partially applied. The different shades of the ground after close feeding and mowing, will also show the parts which have had the most water, and where it has been deficient.

Workmen who have been accustomed to the mowing such crops, can also tell all those parts by the different cut of the grass. Much of the perfection of a water-meadow also depends upon the care and pride which the drowner takes in doing his work well. It would therefore be very advisable not to change those men too often, but to keep the water-meadow constantly under the care of the same workman, so long as he manages it well; and no one should ever alter the water but him who has the constant care of attending it. Water-meadows will never be brought to perfection in any country, till the proprietors and managers of them shall take a pride in doing them well, and strive to rival each other in excellence. Land-owners and agricultural societies should therefore offer premiums for the greatest produce that can be obtained from a given quantity of water-meadow, and a smaller premium to the drowner or managing man. This would excite emulation, and create a conversation and rival spirit of industry, and attention to a pursuit which many might not otherwise have thought about; as the crops of water-meadows are much more at the command of the farmer, and less subject to blight, drought, or uncertainty of season, than any other crop which he cultivates; this would be a fair subject of competition in the skill of the managers, and the premium should not be determined by the produce of a single crop, but by the aggregate produce of the whole year, taken in spring-feed, hay, and autumn-feed.

Account

Account delivered to Mr. Smith, by order of his Grace the Duke of Bedford, of the produce of nine acres from Prisley Water-Meadow, made out of a Bog.

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1803. March 29.—Stocked it with 12 score of sheep, kept them three weeks.

April 16.—Shut it up for hay.

June 23.—Cut the first crop of hay, supposed to be above two tons per acre.

August 20.—Cut the second crop, supposed to be one and a half ton per acre.

September 16.—Stocked it with four score of fat sheep, three weeks; after that it was pastured with lean bullocks, as long and as often as they could find food.

1804. February 27.—Stocked it with eight score and four lamb-hogs.

April 28.—They have now been nine weeks. This is more than eighteen sheep to an acre for nine weeks. It had more and better water this last winter than the winter before, but from our want of grass upon the farm, we have eaten it longer than we should have done.

June 21st.—Began cutting the first crop of hay, which is a greater quantity than the year before, and a larger proportion of the best grasses.

N.B. At the Woburn sheep-shearing in June, 1805, the above meadow was examined by the Secretary of the Society, when the quantity of the grass upon it was not only found to be great, but the kinds of grass it produced in general, excellent in quality, and appearing, on comparison, to improve every year.

Reference to the annexed Plan of the six Acres of Water-Meadow, on Prisley Farm.—Plate VIII.

1. The main hatch, which, when closed, occasions the water that is to irrigate the meadow, to flow into the feeder which fills the highest cuts, made upon the first eight

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eight ridges, from which cuts it gently glides down the slopes into the eight drains, which unite and discharge themselves under the arch at b.

2. The second hatch, which regulates the water for the ten ridges in the second division of the meadow.

3. The third hatch, which regulates the water for the five ridges in the third or lowest division of the meadow, from whence it falls into the old course of the brook*.

X.

On the Use and Abuse of Popular Sports and Exercises, resembling those of the Greeks and Romans, as a National Object. By SAMUEL ARGENT BARDSLEY, *M.D. From the Memoirs of the Manchester Society, Vol. I.*

(Concluded from p. 222.)

Bull-baiting condemned

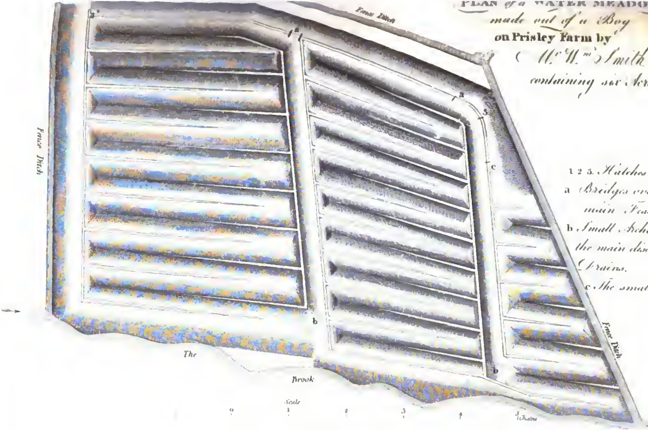
ALTHOUGH persons of rank and education, at the present period, have abandoned bear and bull baiting to the lowest and most despicable part of the populace; and even among them these sports are much less frequent than formerly; yet the practice meets with countenance in some parts of the kingdom, and has been supported not long since, in one of the first assemblies of this nation, extolled by men of rank and abilities, as encouraging harmless amusement, manly spirit, and contempt of danger.

These opinions appear to be so much at variance with the present enlightened, liberal, and humane state of society, that to hear they have been defended by serious arguments and with persevering ardor, is sufficient to excite both regret and astonishment. Cruelty in every shape is unjustifiable; but wanton deliberate barbarity is dishonourable to our nature, and contrary to the principles of natural religion, honour, justice, and humanity.

* A certificate, in terms of high approbation, was given by Mr. Farey, of Crown Street, Westminster.

Of

PLAN of a WATER MEADOW,
made out of a Bog
on Prisley Farm by
(Mr. Wm. Smith)
containing six Acres.



1 2 3. Hatches
a Bridges over the main Feeders
b Small Arches over the main discharging Drains.
c The small Trunk.

Of all the cruel sports, bull-baiting, as generally practised, is, perhaps, the least defensible. It is not only a cruel, but a foolish and detestable diversion. That the spectacle of two animals endowed with courage, strength and activity, exerting their antipathies to each others destruction, upon fair and equal terms, should excite our curiosity and animate our feelings, is reconcileable to the constitution and nature of man; but that any human being should delight in beholding a noble and useful animal tied to a stake, and deprived in a great measure of the means of offence and defence, and then worried and tormented by dogs and men, is a sport so insipid, so unsportsman-like*, and so cruel, as to excite wonder as well as detestation. But the advocates of these and similar cruel diversions, exclaim in a tone of triumphant interrogation——"Do not these sports inspire manly courage and contempt of danger?"—Certainly not. They are only calculated to generate cruelty and a thirst for blood. They may, indeed, inspire ferocity and insensibility to danger, but they are unfit to impart genuine and manly fortitude.

— on account of baseness and cruelty.

The Romans indulged, as before remarked, in these savage diversions to a greater extent than any other nation of antiquity; yet they did not excel the Greeks, nor

The Romans did not derive courage from these cruel sports.

* Throwing at cocks is another specimen of unmeaning brutality confined solely to our own country. After being familiarized to the barbarous destruction of this courageous bird in the cock-pit, it was only advancing one step further in the progress of cruelty, to fasten this most gallant animal to a stake, in order to murder him piece-meal. This detestable barbarity has declined as our manners have become more polished and humane; but the strong hand of the law was obliged to interfere in many places to hasten its abolition. The cruel treatment of the animal race might well lead an ingenious foreigner† to remark, when describing our popular diversion, as follows: "The women of Rome beheld barbarities and murders in cold blood; but the boxing-matches—the bull-baitings, cock-fightings, and the numerous attendance of both sexes at public executions, indicate that there is at least a remnant of Roman manners, and the taste of those times, left in England."

Cock-fighting.

† Wenderborn, on the character and manners of the people of Great Britain.

have surpassed the moderns, in the display of military ardour and true courage.

It is a superficial and unphilosophical view of the subject, to consider the barbarous sports of Rome contributing to the establishment of her power and military fame. These spectacles never became common till after Hannibal's defeat; and that the Romans, subsequent to this period improved in valour and hardihood, is not recorded in the pages of their history. But may we not, with just pride, appeal to facts furnished by our own age and country? Has the valour, enterprize, or intrepidity of British soldiers and sailors shone less conspicuous, since the period that bull-baiting and other barbarous sports have declined, throughout most parts of the kingdom? The answer is recorded in the history of our late naval and military transactions.

Nor are Spain and Portugal exalted for their bull-fights.

The conduct of the Spaniards and Portugueze, when contrasted with that of our own countrymen, is a striking proof of the incompetency of savage and cruel amusements to create a courageous and warlike disposition. Bull-fights still constitute the only active popular amusement of the two countries. If these bloody sports were capable of inspiring active courage and manly fortitude, how are we to account for the acknowledged degeneracy of the people of Spain and Portugal in these warlike qualities?

Arguments with regard to Britain, and the influence of its sports.

The advocates for bull-baiting and similar sports, have recourse to another argument, or rather assertion, which they urge with great confidence: "Cruel sports," they contend, "do not necessarily generate cruelty in a people." "The English, (say they) who are fond of these diversions, are, at the same time, less ferocious, and indeed hold the shedding of human blood more in abhorrence than any other nation on the face of the globe." Granted that we really deserve this honourable distinction—Does it follow that human nature is differently constituted in England to what it is in other parts of the world? Can it be necessary to prove, that habits of indifference to human suffering are acquired by repeated acts of cruelty to brutes; and that the sympathy of our natures must be blunted in proportion to our familiarity with

with scenes of unnecessary and wanton barbarity? These are almost self-evident suppositions; at least they are such inductions from daily and repeated experience, as to pass current for intuitive truths. But if we admit that the English are more addicted to cruel sports, and yet hold human life more sacred than the people of other countries, it by no means follows, that such sports have not a tendency to create a disposition to cruelty. How then are we to reconcile this apparent contradiction? The paradox, if there really be any, is not difficult of solution.

Arguments with regard to Britain, and the influence of its sports.

The life of man is always most respected, where it is of most consequence. For, in a country like Britain, where the whole body of the people enjoy political and civil rights, their own importance, and that of their fellow-citizens will be felt and esteemed; and where just and equal laws protect the life and property of the meanest of the people, and consequently private injuries can be redressed by an appeal to the tribunals of justice, man will be less disposed to be the avenger of his own wrongs. Besides, ignorance is commonly the parent of cruelty. Now it may be safely asserted, that the knowledge of man's duties both towards his neighbour and his Creator, are better understood and more widely diffused amongst the mass of the people in this kingdom, than in those otherwise civilized countries, where a thirst for the blood of their fellow-creatures seems chiefly to prevail. These eminent moral and political advantages are the powerful counteracting causes of that spirit of barbarism which cruel diversions are calculated to excite. If it be desirable then to efface the harsh lineaments of rudeness, and a want of feeling nearly allied to brutality, which still mar the otherwise fair visage of the national character, let all barbarous diversions be entirely abolished; but especially let the sport of bull-baiting be the first offering to be sacrificed at the shrines of humanity and justice! "A diversion," to speak of it in the language of a justly celebrated orator *, "which may be charac-

* Sheridan. Parliamentary Debates on the abolition of Bull-baiting, &c.

terised as inhuman, cruel, disgraceful, and beastly, and which can excite nothing but brutality, ferociousness and cowardice; for, its direct tendency is to debase the mind, deaden the feelings, and extinguish every spark of benevolence."

II. The amusements which depend on bodily exercises and personal contests.

On the art of boxing.

It is not compatible with the limits of this essay to notice the variety of bodily exercises and active sports to which the people of England are generally addicted. But there is one kind of personal contest, to the consideration of which the remaining part of these remarks will be chiefly devoted, as it has been the source of obloquy and reprobation among foreigners, to the national character. The public exhibition of boxing, and the practice of the same art in deciding private and personal quarrels, are here alluded to.

Whether pugilism be commendable

The exhibition of pugilism on a public stage, is most probably a relic of one species of the Roman gymnastic. This mode of venal stage-fighting is a barbarous prostitution of a manly and useful art, whether considered as an exercise calculated to inspire fortitude and intrepidity, or to afford efficacious means of defence against personal insult and violence. But when considered merely in the light of yielding gratification as a public spectacle, or of furnishing an opportunity for gambling speculations, it is then viewed in all its naked deformity—Yet, is not the art of boxing, by which instantaneous insult may be avenged, or personal injury averted, less dangerous than any other practice adopted by the inhabitants of the continent on similar occasions and for similar purposes? The question is an important one; and the following facts and observations may serve, perhaps, to apologize for, if they cannot justify, a custom so interwoven with our national manners and character.

It is less dangerous than most other means of sudden combat.

So long as man is subject to the imperfection of his nature, he must be compelled to acquire the art of self-defence, as well as that of annoyance to others. Our experience of his conduct and character, teaches us the impossibility of extinguishing the passions of pride and resentment, which, although they frequently involve him in

If man cannot be prevented from some appeal to violence, then—

in misery, are still the sources of some of his noblest qualities and attributes. As some portion of evil will attach to the best and wisest system of moral or civil restraint; that policy is, perhaps, the wisest, which legislates for man as he is, not altogether as he ought to be. Suffer the passions to reign uncontrouled, and you dissolve the bonds of society; stifle the active energies of a resolute independent spirit, and you degrade the man into a passive slave. The feeling of resentment for unprovoked injury and insult is a salutary, if not instinctive provision of our common nature. It may be asked—"Is man then to be the judge and avenger of his own wrongs? Is not every offence against the person of a citizen a breach of the laws of society? and should it not be punished as such?"——Certainly:—But if in the best regulated states it be found impracticable to prevent man from frequently asserting a claim to the vindication of his own real or supposed wrongs, it then becomes a question of *expediency* as to the most preferable mode by which he may be enabled to obtain this end. Boxing may not unjustly be considered as the most eligible means of offence and defence. It is properly ranked among those athletic exercises, which, at the same time that they impart address and strength to the body, inspire courage and fortitude in the mind. It may indeed lead *bad* hearts and *bad* heads into acts of presumption and petty tyranny; but this propensity to an improper exertion of skill and courage would be checked, in proportion as men were more *equally* possessed of the means of defence or aggression. They would learn to respect the skill and bravery of each other, and consequently be less prone to undue resentment and quarrels. The government that would attempt, with a despotic and severe authority, to controul the exertions of self-confidence, and a moderate exercise of just resentment, could only expect to rule over a nation of timid and revengeful slaves. The open and ingenuous expression of manly indignation might be repressed; but the rancorous feelings of malignant revenge would be fostered and encouraged. But no state can, with any prospect of success, attempt such an absolute dominion over the passions of men. And if it did,

— boxing is most eligible, for various reasons.

"it

"it must (according to the observation of a spirited author) in order to act consistently, prohibit the use of knives, hatchets, and even pokers; for any of these, upon a sudden emergency, might impart a fearful power to the enraged and the feeble."

Other countries use more destructive means.

If we consider the practice of other countries, where boxing is unknown, we shall find, that the modes of resenting injuries, resorted to by the common people, are full of danger and ferocity. In Italy *, the stiletto is not only the weapon of the hired assassin, but is also kept ready in the bosom of the respectable citizen, to be plunged into the heart of his friend or neighbour, upon any sudden provocation from anger, or motive of revenge.

Destructive effects of want of regulation in personal struggles.

When the passions are under greater restraint, from the influence of laws, of climate and of custom, such dreadful consequences do not ensue from the quarrels of the populace †. Yet even in France, and most parts of Germany, the quarrels of the people are determined by a brutal appeal to force, directed in any manner, however perilous, to the annoyance or destruction of an adversary. Sticks, stones, and every dangerous kind of weapon, are resorted to for the gratification of passion or revenge. But the most common and savage method of settling quarrels upon the continent is the adoption of the Pancratium. The parties close, and struggle to throw each other down; at the same time the teeth and nails are not

* In an authentic publication of the life of the late Pope, it is affirmed, that upwards of 1000 persons annually fall victims in Rome to the stiletto; either by the hands of the hired assassin, or in private quarrels. Dr. Moore reckons the number of murders in Naples, by the dagger, at not less than 400 annually.

† The mode of fighting in Holland, among the seamen and others, is well known by the appellation of Snicker-Snee. In this contest sharp knives are used; and the parties frequently maim, and sometimes, destroy each other. The government deems it necessary to tolerate this savage practice. Certain fines are imposed if wounds be inflicted on dangerous parts of the body; but a very trifling, and indeed seldom any punishment ensues, provided the general rules of the combat have been adhered to.

unemployed.

unemployed. In short, they tear * each other like wild beasts, and never desist from the conflict till their strength is completely exhausted; and thus regardless of any established laws of honour which teach forbearance to a prostrate foe, their cruelty is only terminated by their inability to inflict more mischief. And yet superficial observers, and especially all foreigners who have written concerning our customs and manners, loudly brand the English character with savage rudeness and brutality, because they have seen men terminate their quarrels by an appeal to boxing; in which the parties are not permitted to take an unfair advantage of each other, but when one is disposed to yield, the combat immediately closes, and the conqueror and the vanquished are often seen to give and receive a hearty shake of the hand, in token of mutual good will and forgiveness. In no instance does the manly, spirited, and generous character of Britons, rise to a higher pitch, than in this alacrity almost universally shewn by the most ignorant and lowest order of the people, to terminate their personal contests in a kindly and honourable manner. The mind indeed is thus relieved at once from the brooding mischief of malice and revenge. For, when the idea of self-consequence has been maintained, in courageously supporting the contest, man is better satisfied with himself and others, and consequently more likely to dismiss his ill-will and resentments. In order to foster manly fortitude and vigour, and to prevent the mischiefs arising from the irregular and brutal exertions of strength and ferocity—would it not be advisable to encourage the art of boxing with mufflers, as a subordinate branch of the gymnastic exercises? All *stage* exhibitions of *prize-fighting* ought

In boxing no unfair advantage is ever allowed.

* In Virginia and the other southern states of America, the most savage acts of barbarity are committed, in the quarrels of the people. Gouching—or thrusting out the eye from the socket, is one of the means resorted to upon almost every personal dispute. An intelligent traveller, Mr. Weld, declares, that at Richmond in Virginia, it was nothing uncommon to meet with persons deprived of one or both eyes from this horrid practice. He mentions another mode to disable an antagonist, so detestably barbarous, as to excite incredulity, if the account had not been corroborated by other writers.

to

to be rigidly prohibited; nor should men ever be suffered to prostitute their strength and valour for the sordid purpose of gain.

Where boxing is not used in our country, there is a greater use of barbarous sports.

It is a singular though striking fact, that in those parts of the kingdom where the generous and manly system of pugilism is least practised, and where, for the most part, all personal disputes are decided by the exertion of savage strength and ferocity—a fondness for barbarous and bloody sports is found to prevail. In some parts of Lancashire *bull-baiting* and *man-slaying* are common practices. The knowledge of pugilism as an art is, in these places, neither understood nor practised. There is no established rule of honour to save the weak from the strong, but every man's life is at the mercy of his successful antagonist. The object of each combatant in these disgraceful contests, is, to throw each other prostrate on the ground, and then with hands and feet, teeth and nails, to inflict, at random, every possible degree of injury and torment*. This is not an exaggerated statement of the barbarism still prevailing in many parts of this kingdom. The county assizes for Lancashire afford too many convincing proofs of the increasing mischiefs arising from these savage and disgraceful combats.

Murders prevail more in the Northern Circuit, where they do not box.

The Judges, on these occasions, have frequently declared in the most solemn and impressive charges to the

* A disgusting instance of this ferocious mode of deciding quarrels, was not long since brought forward at the Manchester sessions. It appeared in evidence, that two persons, upon some trifling dispute, at a public-house, agreed to lock themselves up in a room with the landlord and "fight it out" according to the Bolton method. This contest lasted a long time, and was only terminated by the loss of the greatest part of the nose and a part of an ear, belonging to one of the parties, which were actually bitten off by the other, during the fight. The sufferer exhibited at the trial, part of the ear so torn off; and when asked by the counsel, what had become of that part of his nose which was missing—he replied, with perfect naiveté—"That he believed his antagonist had swallowed it!!" It has happened to the writer of these remarks to witness, in more than one instance, the picking up in the streets, lacerated portions of ears and fingers, after these detestable and savage broils. Surely either our laws or manners might interfere in suppressing such deeds of savage barbarity!

Grand

Grand Jury, that the number of persons indicted for murder, or manslaughter, in consequence of the bestial mode of fighting practised in this county, far exceeded that of the whole Northern* circuit; and that, in future, they were determined to punish with the utmost rigour of the law, offenders of this description—But, alas! these just denunciations have little availed. Is it not then highly probable, that the evil which the severity of the law has been unable to correct, might be gradually and effectually abolished, or at least greatly mitigated, by the encouragement of a more manly, and less dangerous mode of terminating the quarrels of the populace? In the Southern parts of this kingdom very rarely (and then chiefly in pitched battles for gain) is there any danger to life or limb from the practice of fair boxing. If then in the public schools and large manufactories of Lancashire, where immense numbers of boys are under the entire controul of their masters and employers, some pains were taken to introduce the manly system of boxing, and the laws of honour, by which it is regulated, there can scarcely be room to doubt, but that the life of man would be more respected—barbarous propensities subdued, and the present character of the county rescued from the stigma of savage rudeness. It has been asserted, by those qualified to judge, that since the late diffusion of the knowledge of the pugilistic art by itinerant practitioners among the Northern inhabitants of this kingdom, the mere exertions of brutal strength and ferocity have somewhat fallen into disuse, both as exercises of pastime, as well as means of offence and defence. In order therefore to abolish all traces of the savage mode of contest which has been so fully described, would it not be adviseable to hold forth prizes, at wakes and public amusements, (where the populace assemble chiefly for the purpose of diversion and pastime) for the encouragement of those, who excelled in sparring with mufflers? This trial of skill, force, and agility (which was at first the practice of the antients) would contribute, *under due regulations*, to

It would be adviseable to introduce boxing into the manufacturing towns, &c.

* At one assizes, no less than nine persons were convicted of manslaughter, originating from these disgraceful encounters.

invigorate the body and animate the courage; and effectually abolish the present dangerous and inhuman method of deciding personal contests.

XI.

Extract from a Memoir of Vauquelin, read to the French National Institute, on the Chemical Properties of Oisanite, compared with those of Titanium, and shewing that the former is the first species of the latter.*

Suspicion of analogy between oisanite and titanium.

SEVERAL years ago, M. Vauquelin indicated an analogy between oisanite and titanium; but as his opportunities at that time permitted him to make experiments upon small quantities only of the oisanite, and the crystalline form seemed inimical to his conjecture, an uncertainty remained which rendered him desirous of repeating his analysis upon a larger scale. It was not till the present year, that by making an excursion into Oisan, he was able to procure a sufficient quantity of that substance for his purpose.

Suspecting that the difference of form and specific gravity between titanium and oisanite might depend on the state of oxidation, he heated equal quantities by the same fire; but no change was produced in either substance. He simply remarked that the titanium became more decidedly red, which was owing to a small quantity of iron contained in it.

They have the same habitudes with alkalies.

Titanium and oisanite comport themselves in the same manner with the alkalies; they combine with them by the assistance of heat, swell up, become white, and are even in part dissolved when water is added to the combination.

It cannot be doubted but that these substances really combine together, because it is impossible to separate them by water or by any other mechanical means. In this state of combination with alkali, the two substances are

* Journal des Mines, No. 114, June, 1806.

soluble

soluble in acids and form triple salts which are easily decomposed by moderate heat.

After having dissolved titanium and oisanite separately in muriatic acid, he subjected them to the action of various re-agents. He remarked that both were precipitated of a fine blood red, by the infusion of nut galls; but that the solution of oisanite afforded a yellowish brown precipitate with prussiate of potash, while the solution of titanium formed a very deep green precipitate with the same re-agent. The author suspected that this last colour might arise from a portion of iron contained in the titanium; he mixed a few drops of muriate of iron with the solution of oisanite, and he then obtained a green colour entirely similar to that which the titanium had exhibited. After having evaporated the solution of titanium to dryness, he washed the residue with distilled water, and obtained a yellow liquid, leaving behind a white substance insoluble in water and the acids; but this matter being again fused with potash and washed to carry off the excess of alkali, was easily dissolved in acids.

The muriatic solutions afford a red precipitate by galls, but the precipitates with prussiate differ owing to iron in the titanium.

The liquor arising from the solution of titanium evaporated to dryness, precipitated a blueish green with prussiate of potash; the solution of the residue being again fused with potash, was, on the contrary, precipitated of a yellowish brown, like that of oisanite, by the same agent and not green as before.

They do not differ when purified.

By this operation, M. Vauquelin having separated the iron from titanium, the solution of the latter then presented absolutely the same properties as those of oisanite. Hence he concludes that the only difference between oisanite and titanium consists in a small quantity of iron and of manganese contained in the latter; but he doubts whether these substances be the cause of the form which distinguishes the oisanite; for these impurities are very small in quantity and may be for the most part separated by muriatic acid.

Oisanite is therefore the first species of the genus titanium.

From these experiments of Vauquelin, it is necessary that oisanite should be removed from the class of stones and placed in that of the metals, under the genus titanium, of which it ought to constitute the first species.

The editors of the Journal des Mines remark in a note that M. Hauy had already conjectured, as was announced in their 61st number, that oisanite, which he denominates anatase, must include a metallic substance. His observations on the form and several other characters of that mineral prove that it constitutes a particular species, which must be separated from the oxides of titanium and placed in the genus of the metal itself.

SCIENTIFIC NEWS.

A Report of the Transactions of the Class of Mathematical and Physical Sciences of the National Institute of France for the preceding Year, was made at the public Meeting of the 7th of July last, of which the following is an Abridgement.

Topography of Persia, extreme dryness and sterility of the soil, &c.

M. OLIVIER presented an account of the "Topography of Persia." He has described the chains of mountains, the courses of the streams, and the productions peculiar to the climate. The nearly absolute drought which prevails, is the cause why not more than one twentieth part of this vast empire is cultivated. Entire provinces have not a single tree which is not planted and watered by the hands of men. This evil constantly increases by the dilapidation of the canals by which the water from the mountains was formerly conducted to the lands, and the territory becoming impregnated with salt, becomes eternally barren.

Conjectures of Lacepede respecting seas and lakes in the interior of Africa.

The reflections of the studious and sedentary cultivator of natural history, may lead to results well calculated to divert the pursuits of travellers. M. de Lacepede, by examining what is at present known of Africa, by comparing the volume of the rivers which arrive at the sea, with the extent of the regions upon which the rains of the torrid zone fall, and the quantity of evaporation to be observed, and lastly, assisting his judgment by the number and direction of the chains of inland mountains, as described by travellers, has offered some conjectures respecting the physical disposition of the countries

countries still unknown in the centre of that quarter of the globe, and more particularly the seas and great lakes which he thinks must there exist. He has indicated the courses which appear to him to be proper for most speedily exploring those countries which still remain to be discovered.

Diminution of the Caspian Sea. Its antient state.

There is another description of conjectural geography, which seems to determine the antient state of places, from what is at present to be seen. M. Olivier has in this manner examined how far it may be admitted that a communication formerly existed between the Caspian and the Black Sea. He thinks it existed to the north of Caucasus, and that the alluvions of the Couban, the Wolga, and the Don have interrupted it.

Since that time, the Caspian not receiving from the rivers which fall into it a sufficient quantity of water to supply its evaporation, has continually sunk in its level, and is at present sixty feet lower than the Euxine Sea. In this manner it is that it has been separated from the sea of Aral, and has left uncovered the immense plains of salt sand which surround it to the north and to the east.

M. Dereau de la Malle, son of one of the members of the Institute, has found numerous testimonies in the Greek and Roman authors of this ancient extent of the Caspian Sea, and his communications with the Euxine and the Aral. He has presented a long memoir on the subject to the Class, and to that of Ancient History and Literature. These researches afford an additional proof of the utility of connecting the exact sciences, with researches of erudition.

Ancient mill stones.

M. Monges has given some observations on two antient mill stones, dug up near Abbeville, from which, as well as from examination into the writings of the antients, he determines, that they made their mill stones, in general of porous basaltes.

Piece goods formed by the loom in antient times.

M. Desmarets, from an examination of some antient garments, found in a tomb of the Abbey of St. Germain, has determined that most of the processes of weaving, at present used, were known in the 10th century, and he has thrown new light upon the articles of Pliny respecting the antient fabrics.

Several

Botanical works.

Several important botanical works have appeared. The Flora of New Holland, by M. de la Billardiere; the magnificent description of the Garden of Malmaison, by Ventenat, have arrived at their nineteenth livraison. The Flora of Ovare and of Benin, by M. De Beauvois is at its fifth. A fifth volume of the Botaniste Cultivateur of Dumont Courset has appeared, and M. La Marck has given in conjunction with M. Decandolle, a third edition, greatly enlarged, of his Flora Française.

M. de Beauvois has begun to publish the insects which he collected on the African and American Coasts. Two parts have appeared.

Cuvier on animals without vertebræ.

M. Cuvier has continued the two great series of researches which he has been engaged upon several years, upon animals without vertebræ, and upon the fossil bones of quadrupeds. In the first of these series he has this year given the anatomy of seven genera; the Scylla, the Glaucus, the Eolides, the Colymacon, the Limax, the Limnæ, and the Planorbe. The two first are very little known, even externally, and the author has rectified the false notions of naturalists with regard to them.

In the second series he treats of the fossil bones of bears, rhinoceros, and elephants.

And on the fossil bones of animals.

Two species of bears, at present unknown are buried with tygers, hyenas, and other carnivarous animals in a great number of caverns in the mountains of Hungary and Germany.

Fossil bones of the rhinoceros and elephant.

Bones of the rhinoceros are found in abundance in the uncompact grounds in all parts of the globe, where excavations have been made. The author has collected notices of more than six hundred places of the two continents, where the bones of elephants have been dug up, and very recently the grinders and tusks have been found in the forest of Bondy, in digging the canal which is intended to bring the waters of the river Ourque to Paris. The more we advance to the north, the better is the state of preservation of these bones. An island of the Icy Ocean is almost entirely formed of them.

—belong to extinct species.

These facts were in great part known; but it follows from the detailed comparison made by Cuvier, of the bones of the rhinoceros and elephants at present living in Africa and

and the Indies, with those of the fossil animals, that they are respectively of different species.

The fossil rhinoceros had shorter legs, a larger head, of greater length, the snout being very differently formed from that of the present rhinoceros. The elephants had the grinders, the head, and particularly the alveoli of the tusks very differently constructed, and the trunk had other proportions.

—having peculiar characters.

The author concludes therefore that these two species are extinct, as well as so many others of which he has discovered the bones and ascertained the characters, and of which ten or twelve hitherto unknown to all naturalists, have their bones incrusted in the plaster stones of the neighbourhood of Paris.

—which formerly lived where their bones are now found.

He also thinks that these species have lived in the places where their bones are found, and that they have not been brought thither as has been generally thought, by an inundation, for their bones are not worn by friction.

Chemical news

The chemical transactions of the preceding twelvemonth which bear relation to the Institute, are mentioned in this report. The new edition of Fourcroy's Philosophy of Chemistry, The Experiments of Count Rumford on the Communication of Heat through Water, and on the adherence of the particles of that fluid together, both which have appeared in our Journal, are stated by the reporter. He also gives a short analysis of the Labours of Berthollet on Chemical Statics, which that able chemist still continues to pursue. M. Berthollet, while he shews that very large quantities of carbonic acid, may, by pressure, be united with the alkalis and earths, takes notice at the same time that these combinations are complete throughout, and not that, as is commonly imagined, one of the principles is superabundant, and as it were disengaged while in excess beyond saturation. In proof of this he remarks that the smallest drop of sulphuric acid, added to a sub-carbonate, does not seize upon a portion of free alkali, but immediately decomposes a portion of the whole salt, and disengages the carbonic acid. And so likewise he remarks that the acidulous sulphate of soda effloresces in the air, which it could not do if any portion of sulphuric

Researches on the chemical affinities, by Berthollet.

ric acid were uncombined; for there is no substance in nature which is more strongly attractive of water.

Measure of the degrees of acidity in different acids compared together.

M. Berthollet has established a method for ascertaining the degree of acidity of the acids, and of the alcalinity of the different bases, by the quantity required of each to saturate or completely neutralize the other, so as to give no sign of either acid or alkaline qualities.

He confirms this method by shewing that the proportions of these quantities are constant, and that if, for example, it be necessary to add to any base twice as much of any species of acid to saturate it as to saturate another base, the former of these two bases will require twice as much of any other species of acid for its saturation as the second will require.

Combinations disunited by heat more readily when water is present.

But the degree of resistance to heat does not correspond with this force, and it is more easy for example, to decompose the carbonate of magnesia than that of lime, by fire, though the affinity of these two earths for the acid is nearly the same. This difference arises from the much greater quantity of water in the first carbonate; and other experiments shew that water favours the disengagement of carbonic acid.

Extensive consequences of this doctrine.

The consequences of these facts, with regard to all the branches of chemistry, particularly the theory of analysis are very important. The tables of affinities and great part of the analyses hitherto made are shaken, and experiment proves in fact that most of these results demand further revision. For example, Klaproth, and after him Vauquelin have found one fifth of fluoric acid in the topaz where it was never suspected. This stone must therefore be ranged among acidiferous substances. Another mineral hitherto considered as a stone, namely the oisanite, must be ranged among the metals; (for which see our present number) and various other instances no less striking and important, are given by the reporter.

History of the late discoveries on platina.

M. Fourcroy has given an account of his experiments on platina, with an history of what has been done by others. Of this last, in abridgement, I give the substance without undertaking to examine into the facts and dates myself.

Descotils attempting to discover the cause of the different

ent colours of the triple salts of platina found that the red colour of some of them was owing to an unknown metal. Fourcroy and Vauquelin, by examining the black powder which remains after dissolving platina, and finding that in some experiments, a metallic vapour having a strong smell was elevated, and that in others, the substance was exhibited in a more fixed manner, considered this powder as a new metallic substance, of which they attributed the different properties to different degrees of oxygenation.

But during this time, Mr. Tennant examined the same black powder at London, and succeeded in decomposing it into two different metals, the one fixed, and the other very volatile; and Dr. Wollaston, another English chemist, by examining the solution which was till then supposed to contain only platina, had also discovered two other metals, different from platina and from those which form the black powder.

So that after the long and painful researches of which this singular metal has been the object for upwards of forty years, chemistry has succeeded in developing eleven metallic substances in its ore; namely, platina, gold, silver, iron, chromium, and titanium discovered by Messrs. Fourcroy and Vauquelin in the more or less coloured sands which are always mixed with it, the two new metals of Wollaston, Palladium and Rhodium, and the other two of Tennant, namely, iridium and osmium.

Short descriptions of these are given, which I shall also transcribe.

Description of Palladium.

Palladium is white, ductile, heavier than silver, very fusible with sulphur, soluble in nitric acid, forming a red solution, precipitable in the metallic state by sulphate of iron, and of a dirty green by prussiate of potash; and forming with soda a triple salt, soluble in alcohol. It was for a short time considered as an alloy of platina and mercury.

—of rhodium.

Rhodium is grey, easily reducible, fixed and infusible, colouring its acid solutions of a rose red, which muriate of tin renders very intense; precipitable of a yellow colour, by the alkalis, and not at all by prussiate of potash. Its triple salt with soda is insoluble in alcohol.

— of iridium. Iridium is white, very hard, difficult to fuse, nearly insoluble in nitro-muriatic acid, and not at all in any other; oxidable and soluble by the fixed alkalis; and its oxide is soluble in acids, giving varied and lively colours to its different solutions. These are the red salts which colour those of platina.

— of osmium. Osmium is a metal not hitherto reducible, of which the oxide has the form of a black and very volatile powder, very odorant, very fusible, soluble in water, and rising with it in vapour, and giving a strong smell and taste to that fluid. Its solution assumes a fine blue colour by the smallest quantity of infusion of nut galls.

The singularity of this composition is no less worthy of remark than the sagacity by which it has been developed.

Discovery of chromium in meteoric stones. Chrominm has lately been discovered in the meteoric stones by M. Laugier, and since by Mr. Thenard.

Whether the composition of muriatic acid, announced by Pacchiani, can be depended on. The discovery of Pacchiani, of the formation of muriatic acid, by galvanism, is considered as in want of confirmation, since Messrs. Biot and Thenard did not find it when they took care to make the experiment without the presence of any thing which could afford sea salt. The experiments of Mr. Sylvester, recorded in our Journal, Vol. XV. p. 50, however appear to confirm the fact, and conduce not a little to explain the process, which no doubt must be considered as still enveloped in obscurity.

Curious and useful researches of Biot upon refraction. In a series of researches upon refraction, undertaken in the first instance for the improvement of astronomy, M. Biot was led to avail himself of this action of bodies upon light as a very happy means of analizing transparent substances.

It has been long known that the rays of light are refracted when they pass from one medium into another of different density, and that the refractions of different mediums correspond with their densities, unless they contain some combustible element. These last increase the refraction much beyond what the simple density would have produced.

From this antient observation it was that Newton formed a judgment that the diamond must be combustible, and he even arrived at the almost incredibly acute conjecture,

jecture, that water must contain some combustible matter.

If two substances be mixed of known refractions and proportions, and regard be had to the density of the mixture, the total refraction may be calculated; and, on the other hand, when the refraction of a mixture of which the elements are known is ascertained, their proportion may also be had. Mr. Delambre, in his report, explains the principles of this calculation. The proportion of parts in known compounds, if transparent, may be determined from their refractive power.

Mr. Biot having applied it to mixtures of known proportions, and having always found it just, has made use of it to determine the unknown proportions of other mixtures.

For this purpose it is only needful to fill a prism of glass, under a known pressure, with the substance intended to be examined; or, to form a prism of it, if it be solid, through which a remote object is to be observed; the angle of refraction is to be measured with the circle of repetition, keeping an account of the pressure, the heat, and the humidity of the external air; and this method being susceptible of a precision equal to that of astronomical processes, necessarily surpasses all our chemical processes in accuracy. But it must also be remembered that it is applicable only to transparent substances, of which the principles are known as to their nature or species. Method of making the experiment

It is particularly useful to give perfection to the analysis of gasiform substances, and Mr. Biot has already obtained interesting results in this respect. particularly applicable to the gases.

Oxigen refracts the least, and hidrogen the most, at equal densities. The refractions of the same gas are strictly proportional to its densities when the temperature is constant. Strongly refracting substances appear to owe their force particularly to hidrogen, for they all contain it. Atmospheric air gives exactly, by experiment, the refraction which, according to calculation, ought to be produced by a mixture of 210 oxygen, 787 azote, and 3 carbonic acid. The application of the rule is found to hold not only in simple mixtures, but in more intimate combinations, provided no very considerable condensation has been produced. Thus ammoniacal gas Common air and other gases tried.

produces the effect indicated by the quantities of azote and of hydrogen which enter into its composition; but if the condensation be too great, there is some alteration though very small. Such is the case with water.

Muriatic acid gas.

The examination of muriatic acid gas, made after these principles, shews that its radical cannot be azote; and also that it cannot be an oxide of hydrogen, containing less oxygen than water.

The diamond inferred to contain hidrogen.

The refraction of the diamond being much stronger than that which is indicated for carbon by the refractions of carbonic acid, alcohol, ether, and other substances of which carbon makes a part; M. Biot concludes, that the diamond cannot be pure carbon, and that we must admit at least one-fourth of hydrogen to satisfy the results of the experiment.

The examination of animal and vegetable products has been carried on with activity and effect.

New principle in asparagus.

The crystalline and soluble principle in asparagus, which is neither acid nor neutral, and does not affect the ordinary re-agents, has been discovered by Vauquelin and Robiquet. The account has already been inserted in our Journal. Vol. XV. 242.

Saccharine matter in bile.

M. Thenard, Professor of the College of France, has completely ascertained the existence in the bile of a saccharine matter which serves to keep the oily part in solution. His methods of analysis are such as do great credit to his sagacity.

Component parts of coffee.

Seguin has made experiments on coffee, which he finds to consist of albumen, oil, a peculiar principle which he calls the bitter principle, and a green matter which is a combination of this last with albumen. He finds that the proportions vary in different specimens; that torrifaction augments the proportion of the bitter principle by destroying the albumen; that these two last principles contain much azote; and that the bitter principle is antiseptic. The oil of coffee is without smell, congelable, and white.

Mr. Seguin has discovered albumen in a great number of other vegetables, and most of them contain a bitter principle, in some respects similar to that of coffee.

Albumen in vegetables—

From the remarkable quantity of albumen found in vegetable

vegetable juices which ferment without yeast, and afford a vinous liquor, this chemist was led to inquire whether the albumen might not be of essential consequence to this intestine motion which is still so little understood. He assures us, that having deprived these juices of albumen they became incapable of fermenting, and that having artificially supplied this principle, snch, for example, as white of egg to saccharine matter, the fermentation took place when other circumstances were suitable, and a matter similar to yeast was deposited which appeared to him to be only the albumen which was altered so as to be nearly insoluble without having lost its fermentescible action. Hence he concludes that albnmen, whether animal or vegetable is the true ferment.

—is the principle of fermentation.

Mr. Seguin has further ascertained that albumen is found in three different degrees of insolubility and disposition to become fibrous; that the more it is soluble, the more powerful is its action; that the respective proportions of albumen and of sugar in the different juices determine the vinous or acetous nature of the product of fermentation, the product being more spirituous, the greater the quantity of sugar; and lastly, that most of the fermentable juices contain a bitter principle analogous to that of coffee, which is of no effect in the fermentation, but contributes to the taste and preservation of the fermented liquor.

Interesting facts relating to fermentation.

Tannin, the vegetable principle formerly discovered by Seguin, and of which the character is to form an insoluble compound with gelatine, has been again examined by Bouillon la Grange, Professor at the Napoleon Lyceum.

New facts respecting tannin.

He has found that it has an affinity for the alkalis, the earths, and the metallic oxides, and the faculty of becoming converted into gallic acid by absorbing oxigen.

The tannins extracted from different vegetables vary a little in their composition; and that which was discovered by Mr. Hatchett, of London, in such great abundance in cachou is rather more oxigenated than the others.

An Italian chemist, Morichini, having discovered fluoric acid in the enamel of the fossile grinders of the elephant, analized the enamel of human teeth, and supposed

Fluoric acid in fossil teeth.

posed he had obtained the same principle. Mr. Gay Lussac finds it likewise in ivory, as well when fresh as when in the fossil state, and also in the tusks of the boar.

Messrs. Fourcroy and Vauquelin have repeated these experiments, and have, in fact, obtained this acid from tusks and teeth altered by their continuance in the earth, but not from the same parts when fresh, nor even in those which were fossil and had undergone no change.

The experiments of Vauquelin upon hair have already appeared in our Journal. Vol. XV. 141.

The nature of Roman alum explained.

Clement and Desormes have made experiments to imitate the Roman alum, in which they have perfectly succeeded in the large way. Their method consisted in calciuation and recrystallization, which afforded an alum deprived of part of its superabundant acid. Curaudeau asserts, that it is also necessary that the small quantity of iron usually contained in alum should be oxided to the maximum for this purpose. But in a later memoir Thenard and Roard appear to have completely disposed of the subject. They have ascertained that one thousandth part of iron has an influence on the effect of alum in dying; that the efforts of the alum maker ought to be directed to clear this salt from that minute quantity; that the oxigenation produces this effect by rendering the iron insoluble; and lastly, that well purified alum is of equal value for manufacturing processes with that of Rome.

Fumigation by oxi-muriatic acid.

The application of the oxygenated muriatic gas to prevent the effects of contagion, as pointed out by Guyton, has been strongly confirmed in the hospitals of France; and it is asserted to have produced the happiest effects as a preservative against the yellow fever in Spain.

Anatomical notices.

Many interesting anatomical researches by Turpin, Cuvier, Tenon, Laumanier, Pictet, Duvernay, Damas, and others, have enriched the year preceding the session of July last.

The report concerning the mathematical transactions of the Class of Science was given by Delambre.

The antient measure of a degree in Lapland was erroneous;—

In the question which has arisen on the subject of the new measure of a degree in Lapland, in which the cause of the error committed in 1736 is required to be ascertained,

tained, Mr. Lalande has sought in his long experience for facts which might answer that purpose. He has remarked, that at that time the use of the telescope of verification (lunette d'épreuve) was entirely unknown. This very commodious and simple instrument, which might be supposed of as early an invention as the application of telescopes to sectors and quadrants, is more modern than might be imagined. We possess the advantage of this, as well as of many other articles of daily use, without inquiring after their inventors. It is mentioned for the first time in the edition of Lalande's Astronomy, of 1764. In order to verify the parallelism of telescopes, Bouguer adopted the use of two pins or studs, which were mutually to be changed in place, in order to ascertain whether they had really the same height. He himself made use of a more imperfect method, which is still less entitled than the studs to be put in competition with the proof telescope of Lalande, which is at present universally adopted. We do not know whether Graham had some equivalent method of approximation to verify his sector. Maupertuis makes no mention of any such thing in the chapter wherein he treats of the verification of that instrument, and this neglect may in part explain the error which is imputed to him.

—because the telescope of verification was not then known.

Mr. Legendre has been busied upon a question of importance, though of rare application. His memoir is entitled, "Analysis of Triangles traced on the Spheroid."

Spheroidal triangles treated by Legendre.

The early astronomers who measured the earth with some exactness, considered it as a sphere of immense radius, in comparison with the small intervals they proposed to ascertain. The greatest side of any triangle in these operations does not exceed 60,000 metres, and the difference between such an arc and the right line that would connect its extremities, is scarcely two decimeters, or the three hundred thousandth part. It was therefore, with reason, supposed that triangles of so minute a curvature might be considered as right lined.

In the latter operations wherein it was sought to determine more exactly the difference between the terrestrial globe and a perfect sphere, an attention to accuracy was carried farther. The triangles formed at the surface

of

of the earth were considered as very minute portions of a sphere, which, in all the extent of each triangle was confounded with the spheroid.

Does this supposition, though less inaccurate than the preceding, promise all the precision which it seems fair to expect from it? and since it is a spheroid which is to be measured, why not calculate the triangles as spheroidal? This question is so obvious that it must at once have offered itself to the astronomers charged with the operation, and to each of the learned men, united from the different parts of Europe, to examine and form a judgement of the work which had been executed. In one of the first meetings of the commission, a learned foreigner, M. Tralles, remarked that the bases of Melun and Perpignan could not be simply considered as arcs which should bo throughout in the same place, but as curves of double curvature. This remark was made by Clairaut above fifty years before; but it was always thought that the effect of the double curvature could not become even a little perceptible, unless upon intervals much greater than we can directly measure; and it was concluded that considerations of the spheroid would only add an useless degree of complication to calculations already too complex. In fact, the spheroid differs from the sphere much less than the sphere itself does from a plane. Now the sphericity of the triangles does not introduce any terms into the calculations but those of the second order for the angles, and of the third for the sides. It was therefore natural to think that the terms dependant on the spheroid would be of an order more elevated, and still less sensible on account of their extreme minuteness. But no one yet had written on the subject; it was not to be supposed that the astronomers would rest contented with vague considerations and a simple probability. This point, they inform us will be found discussed in the article "Calculation of the Triangles," in the second volume of the Meridian, at present in the press; in which it will be demonstrated, from considerations of great simplicity and elementary throughout, that the difference between the spherical and spheroidal angles of the greatest of their triangles, is not one sixtieth of a second, and that the double

double curvature does not change the longest of their sides nearly so much as one centimetre. These results are confirmed by anticipation in the learned analysis of Legendre.

[The Conclusion of this Report in our next.]

University of Gottingen.

The foreign Journals give accounts of the new modeling of the constitution of this University, under the Prussian government, which the disastrous events of war have since shaken to its centre. I shall not copy this part of their intelligence, so little likely to be permanent, but shall confine myself to the notice of the first part of the Meteorologic Researches, which Professor Mayer has read to the Society.

Mayer on planetary affinity or influence.

In this paper he treats of the chemical affinity of the heavenly bodies, or the influence they appear to exercise upon each other independent of gravity, which influence is manifested in their atmospheres. He particularly attends to that of the moon upon ours, which leads him to treat of globes of fire, and stones, said to have fallen from the sky. He remarks, that almost all these phenomena have taken place when the moon was near one of its nodes, and in that half of its orbit in which its light is on the wane. In the cases which seem to oppose this observation, the coincidence of the passage of the moon through one of these nodes, with its last quarter, took place the preceding lunation. Thus it was in 1803, in the lunation which preceded that of the shower of stones at L'Aigle.

Academy of Useful Sciences at Erfurt.

Ore of platina.

In the ordinary sitting of the Academy of Useful Sciences at Erfurt, on the 4th of March, M. Bucholz presented a memoir, transmitted by M. Tromsdorff, intitled, "New Experiments to afford a more accurate knowledge of the Ore of Platina." The author endeavours to

reconcile the contradictions of the English and French chemists relative to this metal. After a long series of experiments, he has found that Platina, in its crude state, contains also four other metals, osmium, iridium, rhodium, and palladium, and he gives the characters of each.

Iceland crystal. Professor Bernhardi communicated some researches on the double refraction of Iceland crystal, or the crystallized carbonate of lime, in which he has noted the phenomena with greater precision than has hitherto been done.

Lycopodium. M. Bucholz communicated the results of his experiments on the seeds of Lycopodium, which afford new views respecting this vegetable product. 1. The seed of lycopodium contains one-sixteenth part of a fat oil, of a brownish yellow colour, and soluble in alcohol. 2. A portion of true sugar. 3. A viscid extract, of a brownish yellow colour and insipid taste. 4. The residue, after treatment with alcohol and water, appears entitled to be considered as a peculiar product of the vegetable kingdom. 5. The yellowish appearance of the seeds in this last state, appears to indicate the union of a kind of pigment with the first principles of the seed, or at least a very intimate union of the constituent parts of this seed. 6. It is the oily part that enters into the composition of this seed which renders it so speedily combustible, and causes it so immediately to separate from water.

Wirtemberg.

Prize for the best theory respecting fossil bones. M. Caula, Banker to the Court of Wirtemberg, has offered a prize of 150 florins (13 guineas), for that explanation which shall be judged the most satisfactory on the subject of the fossil bones continually found in the kingdom of Wirtemberg. It is not simply a critical dissertation which is expected on the different opinions relative to the events which may have transported these remains of the animal kingdom into the places where they are now found; but it is most particularly desired that some elucidation should be given from the chemical decomposition and connection of these bones. It is desired also, that a deduction should be made of the characteristic

racteristic epochas of their existence from the geologic proportions of the successive or gradual strata in which they take their origin, in order to establish upon those data a better judgment than has heretofore been made concerning the revolutions our globe may have been subjected to with regard to the animal kingdom. It will therefore be necessary, for the accomplishment of this purpose, to endeavour to collect into determinate species the animal skeletons at present come to our knowledge, to shew the identity and the differences of these skeletons with respect to living animals; and lastly, to assign to the species, considered as extinct, the rank which they ought to hold in the natural history of the animals still existing on the face of the earth.

Planetary Epocha.

M. de Lalande received, in the month of April last, an anonymous letter, from Franckfort, in which it was said that a German of high reputation in several sciences discovered, fifty years ago, a remarkable period of 280,000 years for the return of the six planets to the same point of the heavens, and his opinion thereon is requested to be given in the Magazin Encyclopédique of M. Millin. The number of revolutions found by the German astronomer for each of the planets have been reduced into seconds by Lalande from the revolutions as at present known, and are as under: Planetary epocha of 280,000 years.

Mercury....	1162577	8836135098921.
Venus......	455122	8835595689448.
Earth	280000	8835940680000.
Mars	148878	8835946519500.
Jupiter	23616	8835946544448.
Saturn......	9516	8835946558608.

M. Lalande remarks, that these numbers differ so little that the deviation from the same precise number of seconds in each sum of revolutions is not greater than the uncertainty in the known durations of those revolutions. He therefore considers the return of the planets in 280,000 years as a curious result, and is desirous of knowing the name of him who had the courage to make such long calculations.

Solar Spots.

Solar spots: M. Hultz, a Prussian Astronomer, of Frankfort on the Oder, has published an opinion, in August last, that the sun at that time was undergoing a considerable change. His opinion was founded on a number of spots occupying one-fifth part of its diameter in their length and one-nineteenth in their breadth. These spots varied in their form, and were perceptibly changed in the course of two or three hours.

To Mr. Nicholson.

Sir,

I shall feel particularly obliged to some of your numerous correspondents for information, through the medium of the Journal, respecting the manner of dissolving the elastic gum (*Caoutchouc*) so as to render it applicable to form a coat on silk, &c. for surgical purposes.

I am, Sir,

Your obedient servant,

Nov. 7, 1806. A CONSTANT READER.

RECORDS OF LITERATURE.

The prospectus of a new periodical work has just been circulated, entitled, Records of Literature; it is intended to present a general statement of the progress of knowledge in all its departments; and, by giving a brief account of all works announced or published, to form an Index to the Literature of the World.

I will examine the principal chemical laboratories, and if the subject should appear of sufficient novelty, after the general descriptions of Lewis, Macquer, Lavoisier, Brisson, and others, I will give a notice as requested by our correspondent X. Y.

A

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SUPPLEMENT TO VOL. XV.

ARTICLE I.

Observations and Experiments to shew that the Effects ascribed by Mr. Dispan to the perpendicular Descent of Hoar Frost are not so general as to support his Theory. In a Letter from* John Gough, *Esq.

To Mr. Nicholson.

Middleshaw, Dec. 8th, 1806.

Sir,

An ingenious memoir on congelation in Spring and Autumn appeared in your Journal for November; in which the writer, M. Dispan, mentions certain experiments on the authority of a Parisian philosopher. He was informed by this gentleman, that if several plates of water be exposed to the air in a frosty evening, one of which is covered with a pane of glass, &c. the water in the open vessels will freeze, but the contents of the covered plate will remain fluid. The same person also observed, that if a funnel be suspended in the evening, at a moderate distance, over a plate of water broader than itself,

Dispan on the congelation of water exposed to the air in the night.

itself, a ring of ice will be found adhering to the circumference of the plate in the morning; but the water immediately under the funnel will retain its fluidity.

Experiment to determine whether the freezing is effected by an action perpendicularly downward.

The perusal of M. Dispan's ingenious essay brought several facts to my recollection, which rendered the accuracy of the preceding narrative somewhat disputable; but memory too frequently preserves the outlines of events, while she neglects to record collateral circumstances of importance; I therefore resolved to try the merits of the Parisian's relation by experiment the first opportunity. This design was soon accomplished; for a suitable occasion presented itself on the 21st of November. The evening was perfectly calm; and I found the copper funnel of a rain gauge slightly incrusted with hoar frost at 7 P. M. A saucer, containing water, was immediately placed upon a stand in the middle of a garden, one half of this vessel being covered with a pane of glass, while the other remained exposed, and the distance of ¼ of an inch separated the glass from the water. The evening proved very favourable to the experiment, for hoar frost fell copiously upon slender bodies, such as gates and pales, as the night advanced, but the gravel walks of the garden remained wet at 11 P. M. The saucer was visited at this time, when it was found perfectly covered with a film of ice, and both sides of the glass were incrusted with rime. The film on the saucer grew thicker in the course of the night; but the vessel of water which stood upon the sole of a window in the garden remained unfrozen in the morning.

It did not prove so.

The facts indicate that the superior temperature of a dwelling house impedes freezing on the ground near it.

Were the preceding remarks submitted to the consideration of M. Dispan's friend, perhaps he would say, that the lintel of the window protected the vessel standing under it from the frigorific particles, by interrupting their perpendicular descent; consequently the water retained its fluidity, being sheltered from the influx of those minute bodies which constitute the true cause of congelation. The experiment might also be rejected by the same philosopher not without some shew of reason, for it evidently answered his expectations in one instance, though it disappointed them in another. The same objections may be urged, with equal effect, by those who suppose that

that congelation is caused in Spring and Autumn by hoar frost falling perpendicularly from the atmosphere; but the morning of the 22nd exhibited appearances which reconcile the apparent contradictions of my experiment on very different principles. The roads were dirty, in consequence of a week of very tempestuous weather, and the surface of the mire upon them was slightly frozen; but the small collections of water and mud which were found near the house remained untouched by the frost, even in situations where the eves of the building could not protect them from the perpendicular descent of particles falling from the atmosphere. The foregoing observation induces me to conclude, that the superior temperature of the house counteracted the feeble effects of the frost to a certain distance from itself, and raised the temperature of a vertical stratum of air above the freezing point: now as the vessel stood on the sole of the window, on which my observations were made, it was wholly surrounded by this warm stratum; consequently, the water contained in it could not freeze, while the saucer in the middle of the garden was exposed to a degree of temperature lower than 32°.

Houses and walls do not heat and cool as quickly as the atmosphere.

I am farther convinced of the justice of this conclusion by having frequently found that houses and strong walls do not heat and cool with the same celerity as the atmosphere. This appears to be the reason why slight frosts are found to prevail in Spring and Autumn in the open fields, which do not extend their effects to towns; while, on the contrary, exposed roads are frequently observed to be wet and dirty after the conclusion of a fit of severe weather, at a time when the ground close to high buildings remains frozen. To prove that this was the real cause of the contradiction apparent in my experiment, I repeated it on a subsequent occasion with this addition: two Florence flasks were exposed to the air, one of them being suspended on a tree in the middle of the garden, and the other on the branch of a cherry tree near a pot of water standing in the window mentioned before. The flask in the middle of the garden was soon incrusted with rime, and a film of ice had been formed in the mean time upon a plate of water standing in the open

air; but the vessel on the window sole remained perfectly fluid, and the flask on the neighbouring cherry tree was only wet with dew, being untouched by hoar frost.

The Parisian experiments were probably made in favorable circumstances.

Several of the facts stated above are common occurrences, and they seem to reconcile the opposite results of my experiment with ease; we may therefore say, without hesitation, that the experiments of the Parisian philosopher happened to be made under circumstances which proved highly favourable to his hypothesis. I venture to make this assertion, because a saucer of water, one half of which was covered with glass, evidently comprises all his experiments in one; it also refutes them singly, by shewing that congelation took place under the pane of glass, as well as in the exposed part of the saucer. In order, however, to give an additional proof of a fact, which is almost demonstrated by common observation, I to k the opportunity of a calm evening when the frost was very gentle, and placed a cup of water under a glass bell upon a stand in the middle of the garden; at the same time a circular plate of metal, five inches in diameter, was suspended horizontally over the centre of a vessel of water, which was considerably broader than itself; the distance between the plate and water was about two inches, but neither the bell nor the plate prevented their respective vessels from being covered with ice. It will now appear, that congelation is not caused in Spring and Autumn by adventitious particles of any kind falling from the atmosphere; on the contrary, vernal and autumual frosts must evidently be ascribed to the common effect which is universally produced in water by a temperature less than 32°. What is still more to the present purpose, water may be concreted into ice, in circumstances under which the aqueous part of the atmosphere cannot be converted into hoar frost; so that there is no necessity to employ the latter production as an agent contributing to the formation of the other. The truth of what is here advanced will be proved by the following experiment, which I made some years ago:—

Water will freeze under a cover.

Congelation in circumstances where hoar frost could not be formed.

Having observed a quantity of wet woollen yarn to be frozen, which was exposed to a moderate north wind upon the rails of a wooden bridge, I suspended a thermometer

mometer in front of it, which did not descend lower than 34°. We have here an instance of congelation where hoar frost could not exist; but suspecting evaporation to be the cause, I had the thermometer dipped in water of 36°, and exposed it again to the breeze; upon which the mercury fell to 29.5°, and a film of ice formed on the bulb in a short time. Two small parcels of wet yarn were also placed in the same situation, one of them being inclosed in a corked phial, while the other remained exposed to the wind; the latter soon became stiff, but the former continued soft for the space of three hours.

The theory of M. Dispan not to be relied upon.

The experiments detailed in this letter have convinced me, and the same evidence perhaps may convince some of your readers, that M. Dispan's theory cannot be relied on; for water has been shewn to congeal without the assistance of hoar frost, when the freezing powers of the atmosphere are very feeble; which appears to be an unanswerable objection to the theory in question. As for the phenomenon described by M. Dispan, perhaps it is not very uncommon; at least I have known something similar occur more than once on a part of the river at Kendal, where the current is obstructed by a weir placed a little below a stone bridge. The expanse of water formed by this impediment is occasionally covered with a sheet of thin ice above and below the bridge in the course of a night of calm and moderate frost, when the river is low and nearly stagnant; at the same time no symptoms of congelation are seen under the arches. I have always attributed this singular occurrence to the same cause which preserved my water pots on the window sole from the frost; that is, I have always supposed that the superior temperature of the bridge prevented the two sheets of ice from uniting beneath it, nor could conjecture furnish me with another reason. M. Dispan's memoir, however, suggested new principles; and as there was a possibility of error on my part, I have endeavoured to throw fresh light on the subject by the experiments which are now submitted to your consideration.

I remain, &c.

JOHN GOUGH.

II.

Description of an Eight-day Clock, with an improved Striking Part, by Mr. Henry Ward, *of Blandford, Dorsetshire**.

Improvement and simplification of the striking part of clocks.

THE striking part of this clock is so far simplified, that the whole train of wheels used in common clocks, together with the barrel and weight, are entirely superseded.

The power necessary for raising the hammer is obtained from the pendulum.

A A A A, Plate X. represent the front side of the frame. B, a cock in which rests the pivot of the pallet arbor. C, a brass arm firmly fixed on the same. D, the gathering pallet, and E, a thin plate of brass, both rivetted on the same collet, which turns on a small stud fixed in the arm C; this brass plate has two notches in it, at *a b*, in which acts a slender spring F, fastened to the collet of the arm C, by a small screw, and serves to keep the gathering pallet in its proper position. G, the cock of the hammer-bar. H, the hammer-tail, which acts also as a hook in the teeth of the rack. I, a brass arm, or rather a lever, which lies behind the minute-wheel N, and is fixed with the hammer-tail to the hammer-bar by means of a pin. K, the flirt. M, the rack. N, the minute wheel. O, the hour wheel. The bridge and snail are the same as in a common clock.

The operation of this work is as follows. A pin is fixed in the back of the minute wheel N, and as it revolves, raises the lever I, by which the hammer-tail H is lifted out of the rack, the rack is then at liberty to fall; the lever I, by bearing against the pin, returns gradually, and prevents the hammer from striking the bell.

Before the pin has quitted the lever I, another pin in the front side of the same wheel begins to lift the flirt;

* Communicated to the Society of Arts. See their Vol. XXIII.

when

when raised to a sufficient height, it is let go by the pin, and falls on the gathering pallet D, which forces it into the rack; it is prevented from rising out of the rack by the spring F, having got into the notch *b* of the brass plate E; the pallet immediately acts on the rack; for, as the arm C moves from left to right, it lays hold of a tooth, and carries it along with it by means of the *vis inertiæ* of the pendulum, at the same time the hammer-tail is raised by another tooth of the rack, and on quitting it the hammer strikes the bell; when the arm C returns with the gathering pallet from right to left, the rack is prevented from returning with it by the tooth resting against the end of the hammer-tail, the pallet is then carried over another tooth, and at the next vibration moves the rack and hammer-tail as before; thus they continue to act alternately on each other till the rack is up, and the clock makes one stroke regularly at every other vibration.

Improvement and simplification of the striking part of clocks.

Now, in order to disengage the gathering pallet, there is a pin fixed in the rack at *c*, and as soon as the last tooth of the rack has got past the hammer-tail, the shoulder of the brass plate E, which is rivetted to the pallet, strikes against the pin *c*, and lifts it out of the rack, the spring F jumps into the notch *a*, and prevents it from returning; thus it remains detached, and the pendulum continues to vibrate without any obstruction. The ball of the pendulum weighs about eight pounds thirteen ounces; and the weight 24 pounds. The clock has a dead scapement.

The objection that may perhaps be made to this clock is, that the striking part disturbs the isochronism of the pendulum; but whoever will take the trouble to try it against another pendulum, of the same length, both before and after it has struck, will find no sensible alteration; and even if that were the case, the irregularities would be periodical, and return to themselves every twelve hours.

The advantages which I conceive this clock to have over a common clock, are as follow:

First—That it is not attended with that disagreeable roaring which is frequently heard in the wheels and pinions

Improvement and simplification of the striking part of clocks.

ons of others, and particularly the fly pivots when in want of oil.

Second—That the interval between the strokes is uniformly the same: the case is very different in other clocks, for as they get foul they always strike slower, and more so still when the weather is cold.

Third—That in consequence of its simplicity, it is not liable to be out of repair.

Fourth—That it can be manufactured for considerably less expense.

HENRY WARD.

III.

Description of a Model, for elevating and depressing Water, applicable to the use of Canal Locks, and for preventing the usual waste of Water therein, with Directions for working the same. By Mr. R. SALMON*.

Improvement to save water in canal locks.

IN Plate X. C is supposed to represent a canal lock of the common construction, whose lower gates *i*, *i*, open towards or into the lock, and its upper gates *k*, *k*, open towards the upper or higher level of the canal; D is a hollow caisson, or water-tight chest, which is fitted to a walled chamber or side-lock, so as to move freely up and down therein; *i* is an opening, which forms a connection between the lock and the caisson-chamber, and which can be closed by a shuttle fitted thereto, when required. Four standards, *e*, *e*, *e*, *e*, are firmly fixed on the ground and walls of the lock and chamber; and four posts, *c*,*c*,*c*,*c*, are fixed in the four corners of the caisson; on each alternate pair of these standards and posts the frames *a* and *b* rest, as on so many fulcrums, or moveable joints; the frame *b* (Fig. 1 and 2) has two straight parallel bars of thin iron fixed thereto, and standing up above the same; the frame *a* has two similar bars affixed to it, except that the

* From the Transactions of the Society of Arts, Vol. XXIII. The Silver Medal was awarded for this communication.

the top edges of these are hollowed into a curve, as shown in the figures. B A, is a carriage loaded with two heavy leaden weights, and resting on four low brass wheels, having grooves in their circumferences like sash pullies, to receive the iron bars upon the frames *b* and *a*, so that the carriage can be drawn along upon them; the distance of the axles of their wheels is such, that when the wheels at B rest on the frame over two of the posts *c*, *c*, the wheels at A shall at the same time rest over the other two posts *c*, *c*, as shown in Fig. 1; and when the wheels at B rest over two of the standards *e*, *e*, the wheels at A shall at the same time rest over the other two standards *e*, *e*, in Fig. 2. In order to work the model, the carriage must be brought into the position shown in Fig. 1, and this can readily be done by stops, which are provided in the proper places on the curved bars, for preventing the wheels from rolling too far; as much water must then be poured into the lock C, as will fill it exactly to the black line *d*, *d*, withinside the same; and if the table on which the model stands be not level, small wedges or chips must be put under the model where necessary, until the surface of the water exactly corresponds, all round the lock, with the top water-mark or line above-mentioned; it must likewise be observed, to place the model across the table, so that the weight *h*, when hung over the pulley *f* or *g*, may be at liberty to descend. Then hang the two-pound weight *h*, Fig. 1, by the line over the pulley *f*, at the upper end of the lock; and the carriage, or load B, A, will be drawn forward into the position shown at Fig. 2, and the water in the lock C will pass through the shuttle, to buoy up the caisson D, and its surface in the lock will descend to the lower level. Again, by shifting the weight to the lower end *g*, the load will again be brought back, the caisson depressed, and the water forced through the shuttle, again raised to the higher level *d*, *d*, in the lock, as in Fig. 1.

Improvement to save water in canal locks.

Hence it is evident that the water in the lock, with or without a boat therein, may be raised or lowered, by the application of any force to move the carriage or load, horizontally on wheels. That when it is intended to pass a boat from the upper to the lower canal, the water in the

Improvement to save water in canal locks.

the lock is raised to the top water-level d, d; the upper gates k, k, are then readily opened, and the boat floated into the lock; this done, and these gates shut, the water and boat, by withdrawing the load from the caisson, is lowered to the lower level of the canal. The lower gates i, i, are then opened, and the boat floated from the lock to the lower canal. In this operation of lowering a boat it is evident, that so far from there being a waste of water, a weight of water equal to the boat and its load is raised from the lower to the upper canal; for when the boat at the upper level first enters the lock, its own weight of water is displaced, and forced into the upper canal. And again, when it is floated into the lower canal, as much is again from that canal displaced, and forced into the lock.

On the same principle that water is gained by a descending boat, as above described, it will be observed, that no waste ensues in an alternate passage; and that in an ascending passage, a loss of water equal to the boat and its load only takes place.

It should be understood, that as canals are sometimes more or less full of water, locks on this principle must be constructed to raise and depress, to the greatest extremes that ever happen, from the highest high-water, to the lowest low-water mark, and that being so constructed, they will apply to any intermediate heights; the curved plane a being formed, to adjust and counterbalance the inclination of the wheels on the other plane b, thereby maintaining an equilibrium, at any intermediate height, which the water in the canal may happen to be at.

Having described its manner of operating, I shall explain and compare cause and effect; for which purpose it may be requisite first to state, that the load of the carriage B, A, is fifty-six pounds, which weight, when advanced, presses directly over the parts c, c, c, c, with all its gravity bearing on the caisson; but when the load is drawn forward, it rests entirely on the fixed standards e, e, e, e, and by this change the whole effect is produced.

Now, if the model be set properly to work, it will be found, that a two-pound weight suspended over the pulley

Mr. Salmon's Improvement on Canal Locks.

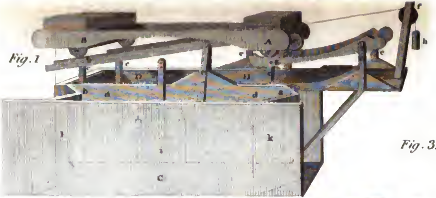

Fig. 1

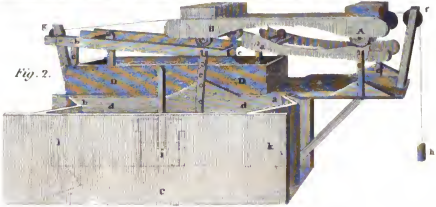

Fig. 2.

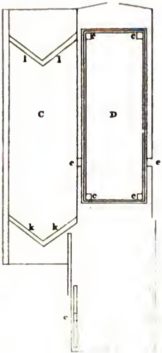

Fig. 3.

ley at either end, will put the carriage in motion, and thereby raise or depress the water in the lock, and that to do so, the two-pound weight will descend sixteen inches. Hence, two-pounds descending sixteen inches, may be denoted the cause or power to produce the effect. Farther, it follows, that this two-pound weight descending sixteen inches produces the same operation as fifty-six pounds laid in the caisson would perform, and this sinking of the caisson D may be denoted the direct effect produced by the two-pound weight. The indirect and requisite effect being that of depressing or elevating the water in the lock C, and the comparison thereon will stand thus: the surface of a body of water, of an area of twenty-four inches by ten, is raised about four inches and a half by the power of two pounds descending sixteen inches; and, *vice versâ*, by reversing the power, the water is again depressed.

Improvement to save water in canal locks.

The shuttle *i*, between the lock and the caisson chamber, will regulate the time of the ascent or descent of the caisson.

R. SALMON.

Woburn, 23*d April*, 1805.
CHARLES TAYLOR, Esq.

SIR,

In reading over the copy of the paper which I hastily drew up, and sent with my model, I observe that I omitted making any remarks on its applicability, improvements to be made in the carriage to facilitate the moving of the load, and on the different other ways, besides the one shewn in the model, by which it may be put in action.

It will readily occur to every engineer, that this sort of lock is not confined to the particular shape of the model, or to any particular form. The caisson chamber may be placed endwise to the lock; may be of any shape, and placed at a nearer or further distance, as may be required.

On comparing the length and movements of the frames in

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in the model, with what may be required in practice, it will appear that the length of timbers at large will not be such but that strength sufficient may be obtained for any load. It is also evident, that, although the frames consist of only two bearers in the model, yet, at large, any number may be introduced, parallel with each other, and as many wheels as bearers.

In this operation the weight of the carriage itself contributes toward the effect, which in common cases is otherwise, as generally there is an objection to the great weight required to make a carriage sufficiently strong for any extraordinary purpose; and there is no doubt but, by an improvement of the carriage, it may be made to require much less than the power used in the present model. The mode I should pursue would be, to make the load in the wheels themselves, that is to say, the necessary load to produce the effect should be two solid iron cylinders, running on as many bearers as are requisite, and to have a frame or carriage for the purpose only of connecting the cylinders; by these means, the strength and friction of the axletrees would be reduced very much, and the means required then to perform the operation would be only to put the body in motion, and to overcome any little obstacle or irregularity, that the peripheries of the cylinders would meet with in their progress.

The advantage of rollers over wheels has been admitted, even where the peripheries of the cylinders were in contact with the incumbent weight resting on the top of them, as well as with the supporting plane below; but, in the case above suggested, they have more advantage, being only in contact with the upholding frames.

With respect to its operation, if any objections should be found to the great animal power that would at large be required, it will occur, that various other means may be used to put the carriage or load in motion; some without any loss of water, and others with a trifling loss, compared with what the lock holds. Thus, when the caisson is up, if by a cock a portion of water be let into it, the equilibrium will be destroyed, the caisson will sink, and the water in the lock be raised. Again, if by a pump, or other means, the water be returned from the caisson

caisson to the lock, the caisson will rise, and the load of itself recede, and this would be without waste of water. To put it in motion with a certain portion of waste, it is presumed, different ways may be found, as the introduction of a portion of water from the upper canal to the lock, or the discharging of it from the lock to the lower level, these would with management occasion the caisson to rise or fall; or, if a part of the load were made to shift farther from, or nearer to the fixed standards *e e e*, it would thereby cause the action required, and perform the operation; and it is probable, that a better way than any here suggested would arise, should the thing be put in practice.

Improvement to save water in canal locks.

I am, Sir,
Your obedient humble servant,
ROBERT SALMON.

Woburn, May 4th, 1805.
CHARLES TAYLOR, Esq.

IV.

Observations, chiefly mineralogical, on the Shetland Islands, made in the course of a Tour through those Islands in 1803. *By Dr.* T. S. TRAILL*.

THE first land we made after passing Fair Isle, was the southern extremity of the Mainland of Shetland. We approached first to Fitful-Head; a bold promontory composed of micaceous schistus. Pass through Cliff-Sound for several miles. The western side of this narrow channel is formed by a chain of low islands, seemingly composed of micaceous rocks. The eastern side is formed by a ridge of hills, which in many places present craggy precipices of the same material. The rocks along this side are all micaceous. Where Cliff-Sound terminates in

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* Communicated by the Author to Mr. Patrick Neill, A.M. Secretary to the Society of Natural History at Edinburgh, from whose "Tour through some of the Islands of Orkney and Shetland" this article is by permission taken.

Scalloway

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Scalloway Roads, I found a micaceous rock, forming the shore for a considerable way, in which there were no particles of quartz visible: it was formed of thin plates somewhat bent or undulated. This kind is reckoned, I believe, very rare; the fresh fracture, if I was not deceived, had somewhat of a silky lustre, and the rock was uncommonly tough.

Part of Scalloway Roads, especially toward the west, is surrounded by micaceous rocks. From Mr. Scott's house, quite through the valley of Tingwall, the general rock is limestone, in which are no vestiges of any marine production, and which, from its vicinity on each side of the valley to micaceous rocks, I suppose, is *primary*. These limestone strata were in most places highly inclined. This valley is the finest in Shetland, both for extent and cultivation. There are two lakes in the midst of it; one of which is said to be fifty fathoms deep. Observe a rude pillar of a single block of granite erected near one of these lakes. Probably it is a Scandinavian monument. Ploughs are more used in Tingwall-parish than in any other part of these islands. All I saw had only one stilt, like the Orkney plough. The spade is much used in Shetland instead of the plough. The harrows I saw here had iron teeth on one side, and wooden teeth on the other.

After traversing half of this valley, ascend the hills to the eastward for Lerwick. Observe fragments of micaceous schistus and granite, as we ascended. Descend toward Elsvoe, by a steep road, where micaceous rocks were prevalent. Observe near the road a vein of *whinstone* traversing these rocks; it was visible but for a short way; it was dense, and dark-coloured. Ascend from Elsvoe a steril hill of micaceous schistus; pass several ridges of mountains covered to a great depth by peat-moss, which concealed all that could interest a mineralogist. On the top of a high hill, find great numbers of rounded nodules of granite, quartz, and micaceous rocks, which the rains have probably separated from a very coarse sandstone breccia, of which this hill is composed. This breccia appears at the surface, as we approach Lerwick, taking place of the primary rocks. The included nodules

nodules become less in size near to the town, where there is no other stone, an argillaceous sandstone excepted, which is much used at Lerwick for building. The hills around Lerwick are in most places absolutely denuded of the peat-moss which had formerly covered them, but which is the common fuel; so that well might the Stirlingshire parson exclaim, "I see nothing but the skeleton of a departed country," when his eyes were directed to this scene. Mineralogy of Shetland.

Return to Scalloway in a day or two. West of Mr. Scott's house, find the micaceous rocks to succeed the limestone; veins of quartz often pervade these micaceous rocks, and sometimes large veins of red felspar. The micaceous rocks are succeeded, as we go westward, by granite, which forms the principal rocks of the western part of the Mainland. On the *Wart-Hill*, find in many places, where the surface was broken, bog iron-ore, arising from the decomposition of vegetables or of the rocks.

Sail by a coast partly micaceous, partly granitic, to Selivoe, where the bay was filled with innumerable medusæ. This name is a corruption of *Silvoe*, which signifies *herring-bay;* but no herrings are now taken in it. Walk from Mr. Barclay's manse to Bixetvoe, another deep bay that intersects this part of the island. The rocks here are of granite, gneiss, and micaceous schistus. These continue round the headlands, in a few places mixed with limestone, as far as Selivoe. The schistus is sometimes formed into millstones. It is curious, that the stones of the hand-mills, now common in Orkney, are of a similar rock, which has been said to have been brought from Norway for this purpose, in ancient times.

In crossing the micaceous hills from Sandvoe to Sensting Manse, find, on the summit of a hill, a large white rock, called *marble* by the natives. It is composed of very large masses of pure white felspar and white quartz, with here and there a little silver-coloured mica. It may be considered as a granite, in which the constituent parts are uncommonly large and distinct. The whole seemed to me to fill a vein in micaceous schistus; but of this I could not be certain, as the hill was thickly covered by turf and short heath.—In my walks around Selivoe, find only

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only primary rocks, chiefly granite and micaceous schistus.

Sail for Foula. Pass grand precipices of red granite. Near the only landing-place on this romantic isle, (the *Thule* of the ancients,) the rocks are all micaceous schistus. North of the landing-place it is filled with garnets well formed, but none of them large. This schistus is of a silver colour for the most part, but I found it quite black in several places. I found also here dark-green hornblende rock in considerable masses. The shores on either hand, as we recede from the landing-place, gradually become bold, and the micaceous rocks give place to tremendous precipices of red granite. The island contains three hills; the highest is about 1100 feet high; precipitous toward the north-west, but sloping toward the south-east. Two of these hills seem as if, in some grand convulsion of Nature, they had been rent from top to bottom, and that one-half had been buried in the waves. The cliffs are very magnificent, and inhabited by innumerable sea-fowl. Among the short heath on the highest hill, find many nests of the *skua-gull* among the largest of the gull tribe, and so bold as to dart at us, and even strike us with its wings, when near its nest. Its colour is ash-grey; its body seems about the size of a small goose; its bill is more hooked than the common gull. The *skua* does not inhabit any other island of this group; it is found at the Ferroe Isles. Observe swallows in the valleys, the only ones we saw in Shetland. The natives say, that their ponies are the best in Shetland. The people seem intelligent and curious. They see the parson only once a-year, when he stays with them some weeks, officiates, baptizes children, and collects his dues. Observe many granite veins traversing the schistus, some of them two feet thick; all are very dense in their texture.

Sail for the Mainland. Pass *Papa Stour*. The north-west coast of this island is of a red colour, but I was not near enough to ascertain the rock. It is hollowed out into grand caverns, through which the waves rush with inconceivable fury, forming a sublime spectacle. Anchor in Hillswickvoe. Sail for Papa Stour in the long boat, but are forced into *Vementry*. Land on a beach

beach composed of rounded nodules of granite, hornblende, and hornstone porphyry. Toward the south end of the island, the rocks are red granite. The end next to the Isle of Mickle Rhoe is partly micaceous schistus, hornstone-porphyry, and hornblende rock.

Pass over to *Mickle Rhoe*, and observe the hornstone-porphyry on the end next to Vementry. A little way from the beach, find a cliff of compact felspar.

The island rises toward the west into vast precipices of red granite, much eroded by the fury of the waves, forming stupendous arches, that mock the feeble efforts of human ingenuity. We observed enormous masses, detached from the island, forming gigantic isolated columns of wonderful magnificence. In a valley that crosses near the middle of the island, find in two or three places black hornblende rock, and hornstone-porphyry, the felspar of which presents regular oval plates, in a dark grey ground, rising through the heath, which was often mixed with *uva ursi* *, used by the natives for tanning. On either hand, the hills rise abruptly, and vast precipices of red granite, entirely destitute of even moss or heath, overhang the valley. In some places, one granite rock was piled on another in horrible confusion, producing, as far as the eye can reach, an indescribably sterile appearance. Barren as this island is, its inhabitants are happy, compared to what they are in some other parts of Shetland. They are emancipated by the proprietor, Mr. Hunter of Lunna †, from the *slavery of fishing*,

* Arbutus uva-ursi. Bear-berry bush.

† *Note by* P. N.—I am sorry that the following extract from a pamphlet, published in defence of the Shetland landholders, should seem to derogate from the praise which Dr. Traill so candidly bestows on this gentleman.

In an " Answer to Vindicator," bearing to be printed at London in 1804, it is stated (p. 45), " Mr. Hunter has found it necessary and convenient to permit his tenants to return to *bondage*, as " Vindicator is pleased to denominate it, under a tacksman; *and it is effected without a murmur!*—I have the evidence for averring, that when Mr. Hunter first proposed this change to his " tenants, out of more than 130 householders, only eight accepted " their *freedom*, and *he was obliged to summon all the rest to remove*, be- " fore

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of *fishing*,—a system fraught, as it is carried on in some of these islands, with the greatest injustice, most flagrant and infamous oppression, which scarcely less deserves the notice of the Legislature, than some branches of *traffic*, that lately accupied its attention. I say this from a con-

" fore they could be brought to try it, even for one year. He then " offered leases to all who would take them; only eight or nine " applied, and a greater number absolutely refused to take any."— " These circumstances," it is added, " are alone sufficient to con- " vince every unprejudiced person of the advantages of the old " system." It appears to me quite otherwise; and I would draw precisely the opposite conclusion. These circumstances seem to afford ample evidence, not only of the extreme indigence, but of the abject dependence of the Shetland tenantry; of the " stupid apathy"—the extinction of the British spirit of independence—which has here been effected by the old system. which must therefore be a bad one. Mr. Hunter, I must remark, seems to have proceeded in his experiment with too great haste, and thus not to have given it a fair chance of succeeding. I do not question the purity of his motives; but I think that it was rather rash, to summon *one hundred and twenty-two poor tenants* all at once to remove! I have been found fault with for endeavouring to subvert the established order of matters in Shetland; but the alterations which I suggested, I proposed should be *gradually* accomplished; I even expressly protested against *precipitancy* My words were, (p. 102,) " In most cases the tenants are so poor, that, were the landlord, *at* " *once* to withdraw his aid, and leave them to manage as they best " could, many of them would probably perish for want." Again, (p. 103,) " Even if the size of farms were enlarged, and leases of " 19 years duration granted, unless manufactures were here and " there, at the same time established, it is not improbable that " many of the present cottars would either starve, or be compelled " to *indent* themselves to America." If, with these moderate sentiments, I incur the charge of being a " bawler about oppression," &c.; with what language of reprobation ought not *Thule*, if consistent, to declaim against his friend *Mr. Hunter*, who, *at once*, subverted the established order of a whole district, and by his *fiat* turned 122 tenants adrift!

The tenants of Lunna, it is stated, submitted to be replaced, under a tacksman " without a murmur." This was about 1803, or 1804. If I knew the author, I would ask him, whether they submitted with equal tameness to the arbitrary increase of the " whale fishing exaction" from one guinea to three guineas—which, as already ob erved, was effected (probably without Mr. Hunter's knowledge) by the tacksman of this district in 1805, but, according to my information, *not* without murmuring!

viction

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viction of its truth. It is not now general: there are several proprietors in different districts, who have emancipated their tenants; but still it is in some places carried on, and prevents my giving unqualified praise to a people, among whom I met with the greatest hospitality and kindness.

The valley terminates in lofty cliffs of red granite. The boat waited for us at a small beach, covered with granitic sand, over which two vast granitic rocks impend, which formed a grand, but rugged vista of naked rock, as we put off shore.

Arrive again at Hillswick-voe. Walk to Hillswick-ness, a promontory chiefly composed of silvery-coloured micaceous schistus, containing immense quantities of garnets, of a very large size: those that were in the upper layers were much decomposed; but below some of them were complete, and finely crystallized. On the west side, this bold headland is perfectly precipitous; but on the east side, in one place, it slopes toward the shore. At this slope, observe a vein of a light-green stone, (probably schistose talc), traversing gneiss. This vein contains most beautiful specimens of common actynolite, some pieces in fibres, others in pretty distinct six-sided prisms; in some cases approaching in lustre to glassy actynolite. The actynolite is imbedded in talc, and was found mixed with steatite. The serpentine is called *kleber* by the natives, who use it as an excellent substitute for metallic oxides in ointments. They apply this ointment to burns with success. Near this place, find black hornblende rock of great hardness; sienite, in one place, containing a large mass of silky-white felspar. A reddish-coloured hornstone-porphyry, in rounded masses, was scattered on the shore. Observe great veins of granite in some places, traversing the micaceous rocks. As we approach the junction of this promontory with the Mainland, gneiss is found succeeding the micaceous schistus. Some grand pillars are detached by the fury of the Atlantic from the sides of this *ness*; the height of these is equal to that of the adjacent cliffs, which impend so over their bases, as to impress the spectator

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tator with sublime emotions, not unaccompanied by fear.

Set out for *Rona's Hill*, the highest point of Shetland. Walk over a granite country to Rona's-voe; cross this long and narrow voe, and land at the foot of precipices of red granite, in which the hill terminates toward the south and west. The hill is at first heathy, but toward the top it becomes naked rock. Its top is a long ridge, covered with fragments of decomposed granite. I attempted to measure its altitude by a portable barometer. I observed the barometer accurately when at the sea-side, both before and after my ascent, and found it stood exactly at the same height at each time, from which I concluded that no material alteration in the pressure of the atmosphere had taken place during my stay on the mountain. The barometer fell when on the summit 15 tenths of an inch, but I had no thermometer, which is necessary to perfect accuracy *. From Rona's Hill, see to a vast distance around,—all the Mainland, near seventy miles long, Foula, Fetlar, Yell, Unst, &c.

Sail close to the promontory of Hillswickness, and observe a great many reddish veins, traversing the micaceous rocks which compose these awful cliffs. Some of them were apparently several yards in diameter.

Pass at some distance a stupendous, insulated, and inaccessible rock, called the *Drongs*. It appears somewhat like a vast ship under sail. It is of a red colour, like some granite cliffs at a considerable distance on the Mainland, the nearest rocks on shore being micaceous.

Pass Isle of *Doreholm*, another insulated rock, perforated by a magnificent natural arch, through which the distant shores of the Mainland were visible. The colour of this is similar to that of the Drongs. Both are probably either granite or wacken, similar to what Professor Jameson describes as found in Papa Stour. A sailor who had been the day before on the shores of the Mainland nearest Doreholm, brought me fragments of both granite and wacken, of a brick-red colour.

* Supposing the temperature 50°, the height here indicated was about 1400 feet.

Observe

Observe that the parish of *Northmaven* (which was not visited by Professor Jameson), is bounded toward the west by tremendous precipices of granite, similar to what compose Rona's Hills, presenting a strong barrier against the encroachments of the Atlantic ocean. Mineralogy of Shetland.

Pass *Ossa skerries*, lofty insulated rocks, apparently of reddish granite. Pass in a fog the Isle of Yell. Double *Ska*, the most northern point of his Majesty's European dominions. It is a small island, composed of gneiss, which forms shores of considerable boldness, and is only at a little distance from the Isle of Unst. Anchor in *Balta Sound, Unst*.

The shores around this fine bason are entirely composed of serpentine rock, and the beach is covered with fragments of the same. The neighbouring hills, some of which are of considerable height, are also serpentine, and in many places are totally divested of vegetation, (even of lichens), presenting to the wearied eye a naked waste, of an iron-brown colour. The shores, from Balta Sound to Norwick Bay, rise gradually into vast cliffs, all of serpentine, in which are frequently found veins of talc, lamellar actynolite, and common actynolite. Observed imbedded, in one place, a substance very like *Labrador Hornblende*, but was not able to force out a single good specimen, on account of the hardness of the serpentine matrix. In the bottom of the Bay of *Norwick*, the shores are low, and a curious striated micaceous schistus presents itself. The striæ are in parallel straight fibres, of a grey colour, with but little lustre, intermixed with small particles of quartz. Near the junction of the serpentine and schistus, close by the sea, in a serpentine rock, find fine specimens of talc in a vein. This vein also contained tremolite in quartz. The serpentine hitherto mentioned has an iron-brown colour, from exposure to the air; but the colour of a fresh fracture is generally of a dark-greenish grey. The striated micaceous schistus begins in the bottom of the bay, and forms part of the western side of it, rising into lofty cliffs, when it is succeeded by a rock containing large masses of whitish felspar, often crystallized in rude rhomboidal figures. This compound rock is by Mr. Jameson called *gneiss*.

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gneiss. This rock constitutes the coast as far as *Burra Frith*, a bay very bold and broken on the east side, where there is a hollow called *Saxe's Kettle*. It is formed by an enormous mass, that seems as if separated from the Mainland, and afterwards joined at its extremities by the falling in of less masses. In bad weather the waves are driven with violence through a small opening toward the bottom, and fill the whole yawning chasm with foam.

The hills that lie between Norwick Bay and Burra Frith are composed to the top of the striated micaceous schistus above mentioned; and, though the highest on the island, are covered with coarse grass and mosses, while the serpentine ones, though inferior in height, are, for the most part, destitute of vegetation. Does not this imply the hostile nature of magnesian earth to plants in general?

At the bottom of Burra Frith, the same kind of undulated micaceous schistus, before seen near Scalloway, again presented itself. From Burra Frith, the coast westward is composed of gneiss and micaceous schistus. At *Hermaness*, the latter rock abounds, and often contains finely crystallized garnets of a large size. Saw one at a gentleman's house found there, which was nearly 1½ inch in circumference, beautifully crystallized, and of a pretty good colour. At Hermaness are said to be grand caverns, into which the tide flows, and which contain fine natural pillars. These pillars are conjectured by Mr. Jameson to be of *gneiss*. The heavy surf prevented me from exploring these caverns. Toward the south, the isle of Unst is less bold on its shores, and the rocks above described are succeeded by argillaceous schistus and sandstone. In crossing the island to Mrs. B.'s, find in the declivities plenty of bog iron-ore, and in one or two places both earthy and schistose chlorite.

The little island of *Balta*, forming one side of Balta Sound, is composed of serpentine of various shades of colour.

Sail for Lerwick: pass the bold coasts of Yell and Fetlar, and sail between Out Skerries and Whalsey; (for an account of these see Professor Jameson's Outline). Sail close under the stupendous *Noss Head*, a grand promontory

montory on the east coast of Noss Isle, composed of sandstone of different hues, hollowed out below into innumerable caverns, the retreat of myriads of sea-fowl, whose various pipes sound harsh discord when heard alone, but when united, form a solemn concert, a tribute of gratitude for that portion of happiness they enjoy. The island of Noss and its holm are composed of sandstone. Over a chasm between the island and the holm a strong rope is stretched, on which a basket is slung, in which the natives pass over to plunder the nests of the sea-fowl that inhabit the holm, and to carry over a few sheep. There is an incorrect engraving of this place, and the method of passing in the basket, published in Pennant's Arctic Zoology, (and from that copied into the Encyclopædia Britannica), from a rude sketch taken by the late Mr. Lowe, an Orkney parson. Mineralogy of Shetland.

Anchor in Lerwick Roads: pass over to *Brassa*, an island composed of sandstone, and of a coarse breccia with a sandstone base, like that already noticed in the neighbourhood of Lerwick. The eastern shores of this island, where they are exposed to the ocean, are lofty precipices like Noss Head, but the southern shores slope gradually to the water's edge. In Brassa and Noss, the strata are not very much inclined.

Walk along the shore west from Lerwick toward Scot's-hall. The breccia and sandstone continue beyond the north-west entrance into Brassa Sound. The nodules imbedded in the former are larger than in that found near Lerwick. As we go more westerly, the primary rocks again make their appearance. Leave the shore, and cross some hills, on which we observed micaceous schistus, gneiss, and hornblende rock. Descend into the northern end of the vale of Tingwall, where we again find limestone. Return to Lerwick by the manse of Tingwall, and pick up in several places fragments of striated micaceous schistus, but not so remarkable as that found in Unst.

Set out in the long-boat to coast the eastern sandstone shores of the Mainland. The coast from Lerwick for some miles seems to be of sandstone or breccia, and is perforated in many places by caves formed by the sea, and

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and into some of these we rowed for several hundred feet. Soon after, we land, and find a compact limestone, interspersed with veins, or reddish calcareous spar, to succeed the sandstone. As we advanced, the hills on our right became higher, and were composed of micaceous schistus, especially at Coningsburgh. From this point they gradually fell in height, and sandstone of a dirty brown colour succeeded.

At *Sandlodge*, in 1803, (when I was there), a copper-mine was wrought, which has, I understand, been since given up; but which, I have been told, it is in contemplation soon again to open. There was then a small but well-constructed steam-engine on it. The principal shaft was sunk within a few fathoms of the sea. The miners had penetrated to the depth of about twenty-two fathoms, and were but little incommoded with water. The upper rock was sandstone; and below it, at twenty-two fathoms, lay a petrosiliceous, or perhaps quartzy rock, traversed by many veins of brown quartz. This was the greatest depth to which they had then penetrated; and I believe that the hardness and unpromising nature of this rock, was the cause of their so quickly giving up. At that time, there were but two Cornish miners, besides a Cornish *Captain of the Mines*, engaged, and these were chiefly occupied in giving directions to the natives employed to work in the mine. The want of men sufficiently skilled in mining, was certainly one cause of their failure. The principal manager was a partner, who had chiefly directed his attention to the corn-trade, as I was informed, and who was totally ignorant of the art of mining. The principal *lode* or vein lies between the sandstone and the petrosiliceous rock, in a direction from N. E. to S. W. The copper-ore is chiefly green carbonate, and the sulphuret; it is imbedded in an iron-ore, which is sometimes pulverulent, and was called by the Cornish miners *gozzan*. The iron-ore is by much the most abundant. When Mr. Jameson visited this place, the copper-mine was not opened; and he only mentions iron-ores as the product of the mine, which many years ago had been wrought by an English iron company, but afterwards abandoned. It was subsequent to Mr. Jame-son's

son's visit that the copper-ore was much noticed. The iron-ores here found, are, Mineralogy of Shetland.

1. Dark-brown, fibrous, and mamillated hæmatites;

2. Columnar bog-iron ore;

3. Micaceous iron-ore;

4. Iron-ochre of a brown colour;

5. Stalactitic iron-ore, colour dark brown;

6. Earthy matter, much charged with iron, seemingly arising from the debris of other ores.

The copper-ores are,

1. Friable and amorphous carbonate of copper, colour rich green;

2. Beautiful carbonate of an emerald green, crystallized in capillary fibres of a silky lustre, diverging in radii from the centre. This species is found imbedded in iron-ore.

3. Sulphuret of copper, disseminated through felspar in some places, and, in others, in great masses in iron-ore.

The rich carbonates were found near the bottom of the mine. The levels and shafts of the old company seem to have passed within three or four feet of this rich vein, but never to have touched it. I walked through the galleries scooped out in former attempts for about forty fathoms, but saw only little appearance of copper ores, while there was iron in abundance all around. The roads near the mine were all paved with fine iron hæmatites, which the Cornish miners who were there did not seem to regard as of any value, nor indeed almost to know. Some of them imagined it was a new kind of copper-ore. Some pieces of bog iron-ore I had collected, were called *copper-spume* by one of them; hence, it is evident, we cannot trust much to the mineralogical opinions of the generality of miners. From the saline taste of the waters of the mine, and the crust of copper it left on my knife, I proposed to the workmen to try to procure *copper of cementation* in the usual way. This company had already expended between £9000 and £10,000 on the work, and had shipped one or two cargoes of ore; for, when dressed and washed, it was carried to England to be smelted. I was informed, that the best

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best of it sold for £70 per ton. The hills in the vicinity afford both copper and iron pyrites in considerable quantity. Near *Coningsburgh cliffs*, a vein of copper pyrites was wrought a few years ago, which yielded Mr. Jameson 18 per cent. of copper; but it so much decreased in width as they descended, that it was finally abandoned. The appearance of the ores, was judged, by the Cornish miners, to improve as they descended in the Sandlodge mine; and, at their lowest level, the quantity of fibrous malachite, when I visited the mine, was such as to afford a most beautiful spectacle by the light of our candles. They have since, however, I am told, unfortunately met with such obstacles, as to induce them to give up the work. Still, it appears to me, that it would be worthy the attention of some mining company, who had capital and enterprize to prosecute the undertaking.

Rocks of sandstone and breccia from the east coast from Sandlodge to Sumburgh. The micaceous hills now cross the Mainland, toward Fitful-Head; and from Quendal Bay to Sumburgh Head, the chief mineral production is sandstone. At *Quendal Bay*, a copper-mine was discovered several years ago, and was, in 1803, slowly worked by a very few miners. In the tract from Sandlodge to Quendal Bay, there are many indications of metallic ores, chiefly iron.

From Levenwick Bay, sail along the shores of the Mainland to Sumburgh Head, the southern extremity of these islands. It is composed of sandstone cliffs, moderately high. Am informed, that a slate quarry has been lately opened, not far from the top of this promontory—Bid adieu to Shetland.

With regard to the general distribution of the rocks which compose the Mainland, the western side of it is composed of micaceous schistus and granite; and is much more bold than the eastern, which consists chiefly of sandstone, and sandstone breccia. The parish of Northmaven contains most granite; and, if I am not mistaken, Rona's Hill, the highest ground in Shetland, stands in this parish. A similar distribution of the strata is, I believe, pretty generally observed in most countries, but the

the cause has not been well explained. All the theories on the subject are lame and unsatisfactory. In the other Shetland isles which I have examined, the western coasts are generally the most bold, and are composed of rocks more indisputably belonging to that class called *primitive*, than those on their eastern shores. The same remark may be extended to the sister isles of Orkney, and even to Great Britain. Mineralogy of Shetland.

Preston's chart of the Shetland islands, is the only tolerable one we have: but it is inaccurate in the northern part, which, I have been told, he did not live to survey. The southern parts of Shetland were laid down by himself, and are extremely accurate; but the northern parts were carelessly added by some inferior hand at his death. I have even seen a small island or rock that is always uncovered, which is not in the chart at all. Mr. Jameson's small map is pretty correct. It would certainly be worth the attention of Government to cause a nautical survey of these islands to be made, with the same minuteness and accuracy that the Orkneys are laid down in the admirable charts of Murdoch Mackenzie. Pinkerton, in his Geography, seems to have supposed, that the Orkney coasts are as ill laid down as those of Shetland. He says, "We have better charts of the "coasts of New Holland than of the Isles of Orkney "and Shetland." Strange, that he should be unacquainted with *Mackenzie's Charts*, which every vessel that sails the North Sea invariably carries!

V.

Facts toward forming a History of Silver. By Professor PROUST*.

THE muriat of silver is soluble in muriatic acid; it separates from it in octahedral crystals. This solution Muriat of silver soluble in muriatic acid.

* Translated from the "Journal de Physique," March 1806, p. 211.

is decomposed by diluting it with water, and the muriat is precipitated.

Muriatic acid forms a muriat with pure silver, and hidrogen is given out.

This acid, poured on parted silver, attacks it, converts it into a muriat, and a gas is evolved, which forces the cork out of the bottle. This gas can be no other than hidrogen. The acid, which is weakened, holds scarcely any thing in solution, for hidrosulphurated water scarcely changes its colour.

That Bergman should announce this muriat as a compound of 75 parts of silver and 25 of acid, was very excusable; but that moderns, certainly not unacquainted with the term of oxidation, should repeat this—Have they forgotten the oxigen?

Muriat of silver does not act on the crucibles.

The muriat of silver does not penetrate the crucibles, or even their surface. It does not act upon them.

Is not volatile.

I did not perceive that it was volatile. During the first impression of the heat only, and by the assistance of the moisture present, a vapour escapes, which is condensed in the empty crucible inverted over it. This sublimate, as Stahl observed, has the appearance of powdered arsenic; but when the muriat is in fusion, no more vapour escapes, and it remains fixed at the bottom of the crucible. Four drams of fused muriat were kept at a moderate red heat for half an hour in a covered Hessian crucible. The crucible, on coming out of the fire, had lost 6 grains: but it was only because a little of the muriat had made its way through one of those flaws, which frequently occur in them if the paste were not very carefully kneaded before it was applied to the wheel.

Half an ounce of the same muriat, heated the same time in a luted retort, did not attack the glass, or give the slightest indication of sublimate. Sage observed the same.

Transparent, of a pearly grey, exactly like the native muriat.

The muriat heated to this point is transparent; and has a pearly grey hue, by which it is distinguished. With this appearance it is so perfectly like the native muriat, that it is impossible not to confound them together; so true it is, that Nature has not two scales for these combinations.

Crystallizes in octahedrons.

The muriat has a decided tendency to crystallize in octahedra.

octahedra. I have a piece of four ounces, in which a horizontal matrix is formed, that is lined with these crystals. The solution of the muriat in ammonia likewise deposits regular octahedra.

Ammoniacal solution of it keeps long, but being decomposed by a gentle heat, fulminating silver formed.

The ammoniacal solution will keep several years, without the muriat's tending to decomposition: but if a separation be forced to take place, even by a moderate heat, fulminating oxide may be formed. This I learned from the following fact:—I placed a capsule containing some of the ammoniacal solution in a moderate heat, for the purpose of collecting the muriat. The liquor being evaporated to a certain point, and a few grains of precipitate having formed, I took it up by the edge, and with blameable carelessness put it into my other hand. The powder at the bottom immediately exploded, cracked the bottom of the capsule, and blew all the liquor into my face. Happily I escaped with no other mischief than the skin's remaining black for some days.

Method of exhibiting it to advantage.

To form a clear idea of the characters of this muriat, and even to make it known in teaching others, it should be poured when half red hot into a silver basin, and moved about circularly, as you would melted nitre. You may then turn it out in a thin, transparent, flexible plate, which may be cut with scissars almost like the horn for lanterns.

Large pieces may be turned into toys, — but the air changes their colour.

If you let it cool in the crucible, to have it in thicker pieces, they may be turned, and made into snuff-boxes, as Kunckel observed; which succeeds better with large pieces than small. I have plates of it of a reddish tint, and marbled like tortoiseshell; but exposure to the light makes it more and more brown, which lessens the value it would otherwise have in jewellery.

Kunckel's method of reducing it.

Kunckel, in his Chemical Laboratory, gives a method of reducing the muriat, which is expeditious, and not liable to much loss. Put three parts of lead, he says, with one of *luna cornea*, into a retort, and expose them to heat; the lead will be converted into muriat, and the reduced silver will subside to the bottom. The product, however, will not be reduced silver, for it will require cupellation, to separate from it a considerable portion of lead. This process is susceptible of improvement. Three

parts

parts of lead are a great deal too much. The muriat, which I suppose to be melted, should be enclosed in a sheet of lead twice its weight; the lead having been previously reduced from acetate of lead; and subjected to cupellation. Thus 73 to 74 hundredths and a fraction will be obtained: in four operations I could not once get 75 and a fraction, which is the real quantity present; a little silver therefore is lost. I know not whether the muriat of lead which is volatilised contributes to this.

A little silver lost.

Reduction by carbonat of potash unadvisable.

Some recommend the reduction of the muriat by means of carbonat of potash. This method is troublesome, and exposes to considerable loss, on account of the swelling up, running over, and the largeness of the crucible that must be used to avoid this even with small quantities of the muriat.

Pure potash preferable.

Pure potash is preferable, since the reduction takes place without any swelling up; but for large quantities this mode is expensive.

Having had more than a pound of muriate, arising from the repeated precipitations that there is occasion to make in a laboratory, I preferred the following method of Sage.

Sage's method.

This consists in boiling the muriat in an iron pot with a few pieces of iron and some water. The separation takes place very speedily. The liquor, which is a solution of muriat of iron, is to be thrown away; fresh water added; and the mixture occasionally shaken. The silver will be obtained in powder, which requires only to be washed, and melted with a little nitre and borax, to free the silver from a little remaining oxide of iron. If you would satisfy yourself, whether the muriat be entirely decomposed, it will be sufficient to expose the powdered silver to the sun; for if any muriat still remain, the light will give it a violet hue, and gradually darken it.

To detect any muriat left undecomposed, expose it to the sun.

Excellence of this method.

To judge of the merit of this process, I made the following experiment.

In a small silver basin I treated a hundred parts of powdered muriat, well dried, with as much iron filings and a sufficient quantity of water. The operation being finished, the muriate of iron poured off, and the powder washed, I added 18 parts of mercury to collect the silver.

The

The amalgama, being subjected to distillation, gave me 72½, instead of 75¼, which ought to have been the product. I triturated the filings again with a little mercury, and obtained 2¼ more of silver, making in all 75. The loss therefore was only $\frac{1}{400}$, or four grains on the quantity I had used, which was 1600 grains of muriat.

The result shows, that with moderate quantities of muriat the reduction may be made with ease, and without any sensible loss. In the great it is still more expeditious, since all that is necessary is to place the muriat upon lead in a cupel.

The silver reduced by means of potash sometimes retains muriat.

The silver separated from the muriat by means of potash, though well fused, is not always secure from retaining some of it. Forging one day a mass of about eight ounces, I was surprised to see it separate into laminæ under the hammer. On examining it, I perceived some unreduced muriat between the plates. Mr. Fernandez made the same observation, as will appear at the end of this article.

Native muriate in America.

The muriat of silver frequently occurs among the minerals of America: it accompanies the native silver; and is found disseminated among the sulphurets, carbonats, and siliceous rocks, so as to be altogether imperceptible.

Analysis of one from the Caraccas.

I examined an ore from the province of Caraccas, which by the application of acids yielded me only 11 marks to the hundred; but having fused it with oxide of lead, potash, and charcoal, it yielded a button, which left 19 marks in the cupel. This ore contained its riches in two states, in metal and in muriat. The silver of this ore contained likewise a 36th of gold. Its matrix was siliceous.

Mode of discovering the muriat.

To discover the muriat, the ore should be boiled with water, and a few filings of iron or of zinc: the muriat will thus be decomposed, and, after washing the ore, nitric acid will detect the silver. The muriatic lixivium being precipitated with a solution of silver, the portion of muriat of silver collected will be exactly equivalent to that which existed in the ore.

Very pure native muriat from Peru.

Pieces of native muriat of the greatest purity are sometimes brought from Peru. I have one that weighs about ten ounces, the matrix of which is rhomboidal carbonat.

This

This muriat is transparent, pearly, and may be cut without splintering. Its chips assume the violet tint in a few minutes on exposure to the sun. It may be melted in a retort, without losing any of its characteristics, or diminishing in weight. If it be brought near the flame of a candle, it flows in red drops like the juice of a gooseberry, which grow white on cooling. They should be caught on a plate of glass. The artificial muriat yields likewise coloured drops when melted. The native muriat enclosed in a sheet of lead does not yield above 74 and a small fraction of silver, though it contains 75¾.

Analysis of this ore.

I decomposed a hundred parts of this muriat by means of zinc, and precipitated the solution, with nitrat of silver. The product collected was a hundred parts. I had the same result twice. The solution of silver may be employed, which will remain mingled with zinc after the decomposition has taken place; but as the necessity of filtration occasions a slight loss, only 98 or 99 will be obtained. The muriat reproduced should not be weighed, till it has been thoroughly dried.

Mingled with carbonat.

The muriat is likewise found mixed with carbonat of silver in powder, exhibiting a perfectly uniform grey paste. It is very difficult to break. Rubbing it with any hard body will make the silver shine. I have one piece which consists of silver 30, carbonat of lime 32, muriat 38.

Large piece of native silver in the *Cabinet du Roi*.

As far as I can judge from appearances, it is this paste that incloses the celebrated piece of native silver preserved in the King's collection. This piece, weighing near three hundred pounds, is still loaded with part of its valuable coat. If the miner had not robbed it of the rest, it would have been much more precious to the mineralogist. It appears to have been considerable. It was cut off with a hammer and chissel. This specimen came from the mines of Quantacaia, on the borders of the Pacific Ocean.

verta kes from oxides of iron part of their oxigen at a high temperature, and

Silver is likewise among the number of metals capable of taking from iron that portion of oxigen, which raises it from the minimum to the maximum of oxidation, or which is included between 28 and 48 per cent. But for this it must be assisted by a certain degree of heat; for on

on losing this the iron in turn, or its oxide at a minimum, takes the oxigen from the silver. The following is the experiment. gives it back to them when cold.

In a small matras heat a solution of red sulphat of iron on parted silver, a portion of the latter will be dissolved, and the sulphat will become green. Filter, add a little water with salt, separate the muriat, and the filtered liquor, or sulphat of iron, will be precipitated green by alkalis. But if, instead of separating the silver, the filtered liquor be kept, it will exhibit scales of metal, in proportion as it cools. Now this new precipitation cannot take place but by the base of the green sulphat resuming oxigen from the silver. We shall not be surprised at this result, if we recollect, that a solution of green sulphat, mixed with a solution of sulphat of silver, instantly precipitates the metal in a shining powder. I had an opportunity of observing, some time ago, that the solution of red sulphat could not be concentrated in a basin of fine silver without giving birth to similar changes. Proof.

Carbonat of Silver.

This carbonat, obtained by means of that of potash, is of a yellowish white colour, but does not keep well, grows black on exposure to the light, and gradually parts with its oxigen; for if at the year's end very dilute nitric acid be poured on it, a portion of silver in powder will be separated. Carbonat of silver parts with its oxigen spontaneously.

Sulphat of Silver.

If mercury be thrown into the solution of this sulphat, it decomposes it, and the result is a flat crystallization, which scarcely deviates from the level, and of course exhibits none of the pleasing phenomena of the nitrat. Sulphat decomposed by mercury.

Tree of Diana.

Lemery recommends, to throw mercury into a nitric solution of silver considerably diluted; and he is right. In little, or in great, very beautiful and very various trees of Diana will be obtained without fail. Homberg and Beaume, with their balls of amalgama and solution, have only complicated the process, and disgusted those, Arbor Dianæ.

who would enjoy without so much trouble one of the most beautiful results of experimental chemistry.

Acetat of Silver.

Acetat of silver

Distilled vinegar very readily dissolves oxide of silver, and affords long white needles, easily crystallized. Heated in a retort it gives out radical vinegar, some gasses, charcoal, and pure silver. I have not examined it further.

On the Surcharge which the Muriat of Silver is capable of giving to cornets for parting, by Don Domingo Fernandez.

Assays vitiated by employing muriat of silver imperfectly reduced.

In the month of December 1794, having occasion to assay twelve pieces of gold coin, I was surprised to see the twenty-four cornets come out with a surcharge of half a grain above tho fineness they ought to have had. I repeated the assays, with all possible care, and the result was still the same. To satisfy my doubts, I examined particularly the lead and the acids I had employed; but I found nothing in them that could account for a surcharge so extraordinary.

I had no other step to take, therefore, but to return to the silver; but as this metal came from a muriat reduced by means of potash and charcoal, I was far from suspecting that it could have any influence on the results; particularly as it admitted of being drawn out by the flatting mill into very thin leaves without any apparent defects. Unwilling however to announce, that these coins were half a grain above their standard, without a fresh examination, it occurred to me, to dissolve the twenty-four cornets separately in *aqua regia*. The solution was scarcely finished, when at length I discovered the cause of the phenomenon. Each of them let fall a white powder, which I collected for examination; and I had no difficulty in recognizing it to be muriat of silver. That of each of the cornets was precisely the same weight.

After this discovery I immediately examined the quality of my silver. I dissolved some in nitric acid, and the muriat separated from it instantly. Thus it is certain, that this salt is not *always* completely decomposed in reducing it with potash; and that it may incorporate with the

the metal, or even dissolve in it, so as not perceptibly to affect its ductility, since it can be flattened out without shewing any defect in the plates.

I repeated my assays, but with pure silver also obtained from the muriat, and the surcharge disappeared. This observation made me determine to dissolve the cornets, as often I should find reason to suspect an extraordinary surcharge.

On the Nitrat of Silver.

Silver carried off on evaporating its solution.

The acid that rises from a solution when evaporating it carries off some silver, however gentle the ebullition may be. The muriatic acid makes this known instantly. This result, which I have had opportunities of proving several times, cannot fail to diminish a little the confidence we might be inclined to place in assays of ores by means of nitric acid; and we know it is indispensable to boil it long enough to resolve the last portions of sulphuret of silver.

Nitrat contains no water of crystallization.

The nitrat of silver does not appear to contain water of crystallization any more than nitre. It may be kept in fusion in a retort for a considerable time, without losing more than a hundredth of its weight. On cooling it fixes in a crystalline mass of a greyish hue, called *lapis infernalis*. This nitrat, heated till it is completely decomposed, leaves 64 hundredths of pure silver; from which we may infer, that a hundred parts of silver will produce 140 of nitrat [156·25].

State of oxide in the nitrat.

A hundred parts of silver take $9\frac{1}{4}$ or $9\frac{3}{4}$ of oxigen to serve as a base to the nitrat, according to a trial I formerly made, though it would not be amiss to examine the fact again.

Composition of nitrat of silver.

This nitrat then would consist of

Oxide of silver......	69 or	70
Nitric acid	31	30

On assaying Ores of Silver.

Silver ores best assayed in the dry way.

Many authors recommend, to precipitate the solution with copper; but this must be carefully avoided, for whatever care be taken, a little silver will always remain in the liquor, as the muriatic acid will shew. An ore that

yields 10 per cent by fusion will not give above $8\frac{1}{2}$, 9, or thereabouts, by precipitation. The estimation deduced from the muriat of silver too being a language not understood by those, who would have the clear product of their ores set before their eyes, it is better to proceed to the assay by fusion. The following method, an imitation of the operation in the large way, and practised by Sage, appears to me preferable to any other.

The best method, as practised by Sage.

Melt a quintal of the roasted ore with as much litharge, and three quintals of common carbonat of potash, in a crucible, the bottom of which is lined with 24 or 30 grains of charcoal, softened with a little oil, so that the paste may be applied to the bottom and half way up the side by the finger. Put on a cover, but without luting it. Place two such crucibles side by side in a common furnace, and cover them with charcoal. The bellows are not necessary. When the mixtures enter into fusion, which will readily be perceived by the ear, push the charcoal aside, so that you may be able to take off the lids, and see what is going on If the effervescence raise the contents above the middle of the crucible, remove the lids, when the weight of the air will check the swell, and prevent it from running over. As soon as all is quiet, put on the lids again, cover up the crucibles with charcoal, and let them stand till they are cold. If the assays have been well fused, the leads obtained will not differ in weight two grains. Subject them to cupellation, and you will obtain buttons, which ought not to differ a sixteenth of a grain. A sixteenth of a grain represents an ounce in a hundred pounds: but if the ore be so poor as to yield less than an ounce, as is the case with most of the mines at present worked in America, the assay should be made with four hundred grains at least,

Nitrat of Silver at a Minimum.

Preparation of minimum nitrat of silver.

In a solution already saturated boil powder of silver, such for instance as is obtained in ordinary parting; and continue the ebullition for an hour after nitrous gas has ceased to be evolved. Pour off the liquid with its sediment, and leave it to grow clear by subsidence. Draw off the clear fluid with a bulbed siphon, and, if you wish to

to concentrate it, pour it into a retort, at the bottom of which a few bits of pure silver have been previously put; if not, keep it in a bottle.

But the parted silver must be cleansed before it is used, to avoid any mixture of copper. For this purpose boil the powder in a saturated solution of silver slightly diluted, when the copper will be converted into nitrat, and the silver will come out sufficiently pure to effect no alteration in the colour of the fresh nitrat.

Parted silver should be freed from copper.

The solution is invariably of a pale yellow colour; and it may be concentrated much beyond the nitrat at a *maximum*, without fear of crystallization, as the nitrat it affords is infinitely more soluble. When it is in the proportion of 240 to 100 of water it is still far enough from crystallizing, and sometimes it remains fluid for several days: but if it be poured into a phial, it congeals so suddenly, that the last portions from the mouth of the retort become solid like icicles from the eaves of a house, and a great deal of heat is evolved.

Minimum nitrat very soluble.

Suddenly becomes solid.

While this nitrat is concentrating, a little is always volatilized. This passes from the minimum of oxidation to the maximum; and sometimes we discover in it a mixture of the two nitrats. In the first case ammonia does not change the transparency of the product; in the second, the new nitrat is indicated by a black hue.

A little volatilized and changed during evaporation.

It is difficult to bring it to a regular crystallization, because it has a much greater tendency to congeal, than to separate into crystals. If it congeal, it cannot be redissolved, without the separation of a yellow powder, which obliges us to suspend the process, that this powder may be allowed to subside, decant the clear fluid, and return it into the retort. It is true that this inconvenience may be avoided, by an addition of acid to the water with which the mass is to be redissolved; but by this addition we are liable to increase the oxidation of a portion of the nitrat, and to convert the product into a mixture of the two nitrats.

Difficult to crystallize.

Yellow powder produced,

The yellow precipitate we have just pointed out, is a nitrat doubly at a *minimum*, both of oxigen and of acid. It forms because part of the new nitrat cannot dissolve in water, unless it takes from the other a little of its acid; and

which contains a minimum both of oxigen and of acid.

and it is the loss of acid experienced by this portion, that occasions it to be precipitated. These effects are completely analogous to those of the nitrat of mercury at a *minimum* of oxidation when thrown into water. If it do not divide into two parts, and if one do not borrow acid from the other, it does not dissolve: but in the case before us, as well as in that of mercury, a little acid sets the yellow precipitates afloat, it increases their saline state, and in consequence restores their solubility.

Analogous phenomena with mercury.

We may succeed in crystallizing it, however, by successively suspending and resuming the distillation, till by repeated trials we have brought the solution to the proper point. But I have not yet been able to obtain crystals sufficiently separate, and out of the retort, to be able to examine them easily.

Mode of obtaining crystals.

Properties of this Nitrat.

Attracts oxigen from the air,

Its solution kept in the air, and defended from dust, loses its colour, and in a few days affords large square laminæ of the common nitrat, or that at a *maximum*.

and from nitric acid.

It is amusing to observe the readiness with which a few drops of nitric acid, poured into this solution, give rise to large scales of nitrat. The fluid changes from white to yellow, blackens more with ammonia, and at length becomes wholly nitrat at a *maximum*. If the acid be mixed with a more dilute solution, and heated, nitrous gas is evolved, and confirms the change indicated by theory.

Muriat of silver only in one state.

Muriatic acid poured into the solution of *minimum* nitrat affords a muriat, the base of which rises to the *maximum* in the very process. There is no muriat at a *minimum* therefore; or at least I have not been able to form one. The following are some of its most striking habitudes with reagents.

Its effects on litmus,

With water of litmus the solution of common nitrat of silver produces no effect; that of the *minimum* nitrat precipitates a blue lake.

cochineal,

With cochineal the *maximum* nitrat produces a scarlet colour; the *minimum*, a deep violet lake.

indigo,

With solution of indigo in sulphuric acid the *maximum* occasions

occasions no change; the *minimum* deprives it entirely of colour, and the silver is reduced.

The tincture of fecula of hemlock, which is of a feuillemort colour, is not changed by the *maximum* nitrat: by the *minimum* the green is revived and beautified in a striking manner. tincture of hemlock,

With Ammonia.

Ammonia precipitates the *minimum* nitrat black. The precipitate collected is pure silver, and does not fulminate, however long it be kept in the ammonia. The ammonia then holds *maximum* oxide in solution; for if it be saturated with very dilute nitric acid, it is no longer blackened by ammonia. Thus we see, that the portion of oxide dissolved in ammonia is raised to a *maximum* at the expense of that which is precipitated. ammonia,

With pure Potash.

The precipitate is brown, resembling in colour that given by the *maximum* nitrat. Redissolved in nitric acid, ammonia precipitates it black, which demonstrates that the oxide has not altered its state; but on drying it attracts oxigen from the atmosphere, rises to the *maximum*, and no longer differs from the precipitate afforded by the *maximum* nitrat. potash,

With Alcohol.

Alcohol acts on its solution no otherwise than water: part of the nitrat, its portion of acid being diminished, separates in a yellow powder. The alcoholic solution being distilled, leaves behind nitrat at a *maximum* and powder of silver, because part of the oxide has completed its oxidation at the expense of the other. The alcohol is not converted into ether in this case, any more than with the *maximum* nitrat, which it dissolves perfectly well. alcohol,

With boiling Water.

It has been seen, that cold water separates this nitrat into two parts; that one, to enable it to dissolve, was obliged to take acid from the other, which was consequently and boiling water.

quently separated in the state of yellow nitrat, or nitrat with an inferior portion of acid. With boiling water the change proceeds farther.

If a few drops of a somewhat concentrated solution be let fall into a glass of boiling distilled water, three colours will be seen distinctly to succeed each other very quickly, yellow, red, and black. If, when the mixture is yellow, or red, a few drops of acid be added, the whole grows clear, and the change is stopped. If this be done the moment after the black colour has appeared, the acid no longer restores the transparency of the mixture, because the black powder is not an oxide, like the two preceding, but disoxidated silver. Now to dissolve this a stronger acid is required.

Disoxidates the silver.

The black powder silvers the glass as it comes into contact with its sides. None of these effects will take place, if a few drops of nitric acid be added, before the solution is dropped into the boiling water.

Nitrat of mercury has similar properties.

The yellow nitrat of mercury at a *minimum* exhibits similar phenomena, though in a different manner. This nitrat, boiled with water in a retort, affords powder of mercury, which passes into the receiver with the aqueous vapour; that is to say, by the help of a high temperature part of the mercury has a tendency to complete its oxidation at the expense of the other. But in whatever circumstances we discover a metal passing from one state of oxidation to another, we shall never find it stopping at any intermediate term between the two extremes of oxidation proper to it.

Metals never found at an intermediate state of oxidation.

Attracts oxigen from the air but slowly.

An aqueous solution of *minimum* nitrat of silver does not superoxide itself very rapidly by contact with air.

Acids have more affinity for oxides of silver than of copper.

I have formerly shewn, that, if copper have a greater affinity for oxigen than silver has, we must not thence conclude, that acids would have more affinity for oxide of copper than for oxide of silver; and in fact the nitrat and sulphat of silver dissolved and kept on carbonat of copper do not attack the latter, or even become coloured. The *minimum* nitrat of silver is equally void of action on carbonat of copper.

The yellow powder, or *minimum* nitrat with less acid, is equally void of action on the carbonat of copper.

The

The *minimum* nitrat, concentrated in a retort, grows thick, gives out a little nitrous gas, enters into fusion, and affords a yellow sublimate that lines the sides of the retort. The *maximum* nitrat affords nothing similar to this. If the melted mass be dissolved, we see a yellowish precipitate, mixed with a little powder of silver, fall to the bottom; which indicates, that a part of the *minimum* nitrat remains unaltered, and that another portion has assumed the *maximum* state, by means of the oxigen attracted from part of the oxide. Minim. nitrat fuses and sublimes.

Several years ago I remarked this transmission of oxigen from one portion of an oxide to another, in those solutions of silver in which I had carried the ebullition on metal pretty far. A kind of brilliant aventurine disturbed its transparency at the expiration of a few days, and afterwards subsided to the bottom of the vessels. This was the first fact that led me to suspect, that silver, like so many other metals, was susceptible of two determinate states of oxidation.

Nitrat of lead treated with leaves of this metal exhibits results of the same class. I will endeavour soon to lay them before the public. Nitrat of lead affords similar results.

VI.

Description of a Machine for cutting Paper and the Edges of Books. By Mr. John J. Hawkins *.

THE object of this improvement is, to cut the edges of books on three sides at one fixing in the press: to do this, it is necessary that the book be placed at one end, and a support given to the plough beyond the part cut; there is therefore at each corner a block moveable on a centre, so as to elongate alternately the side or end of the press. Machine for cutting the edges of books, &c.

The press is as wide as the intended length of the book.

* Communicated to the Society of Arts, who voted the Silver Medal to the inventor.

Machine for cutting the edges of books, &c.

At a distance from the end of the press, equal to the required width of the book, is a stop, made somewhat like two combs, one fastened on each side, the teeth of one going into the interstices of the other, so that it may effectually prevent the book falling too low, whether the press is open little or much.

In the common press, the book is put in the middle, and there is a screw at each end to force the press together; but in this press, the book being put at one end, there is a screw about the middle to force the press together, and another screw at the lower end to force it open, and consequently press the book tighter, exactly on the principles of cabinet-makers' hand-screws.

Reference to the Engraving, Plate IX. Fig. 1, 2, 3, 4.

Fig. 1 and 2. Two geometrical views of the press.

Fig. 3 and 4. Two perspective views of the same, as placed on its supports in the box which receives the cuttings. The side of the box is represented as taken away, in order to show the parts more distinctly. *Fig.* 3 shows the position of the press, when the front of the book is to be cut, and *Fig.* 4, while the ends are cutting.

N. B. The letters of reference are the same in all the *Figures.*

A. The press.

a. The cutting box.

b b. The elongating blocks turning on their centres (*c*).

d. The screw which forces the press together.

e. The screw which forces the lower ends of the press asunder, and consequently presses the book tighter.

f. A piece of board put into the press with the book, to keep it firm against the knife.

g. The comb-like stop.

h h. Bars fixed across the cutting box, to support the press while the top or bottom of the book is cut.

i i. Bars to support the press while the front of the book is cut. In this position, there is a bar (*k*) to keep the press steady, which is taken out when the position *Fig.* 4 is in use.

l l. Pieces

M.r H. Ward's new striking Movement in a Clock.

Fig. 5.

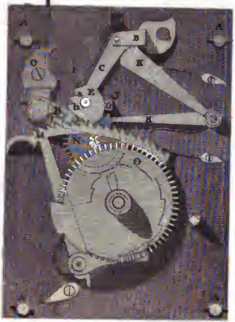


Fig. 2

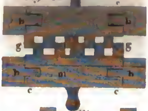
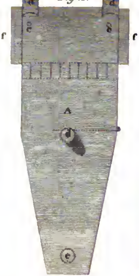


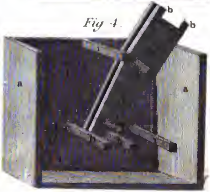
Fig. 1.



M.r Hawkins Machine for cutting the the Edges of Books.

Fig. 3.

Fig. 4.



ll. Pieces of board with notches in them, fastened to the box, to receive the bars.

Machine for cutting the edges of books, &c.

m. Groove for the plough to work in.

It is evident from the positions of the press, that the plough must be worked on an inclined plane; but this, instead of an objection, is a considerable advantage, because the workman has much more power in that direction, than on a level.

The press requiring to be turned round to cut the top and bottom of the book, the plough must be worked left as well as right handed, but this is acquired by a few minutes practice.

The same plough is used as with the common press.

J. J. HAWKINS.

Dalby Terrace, City Road,
May 29, 1805.

CHARLES TAYLOR, Esq.

VII.

Observations on the Permanency of the Variation of the Compass at Jamaica. In a Letter from Mr. JAMES ROBERTSON *to the Right Hon. Sir* JOSEPH BANKS, *K.B. P. R. S. &c*.*

SIR,

General introduction.

AS any improvement, or discovery in the arts and sciences, will, I am persuaded, experience your favourable reception, I have the honour of submitting to your consideration a discovery I have made on a subject, the state of which can only be ascertained by observations made from time to time, as it is not regulated by any known law of Nature; I mean the variation of the magnetical needle.

This discovery may not only excite others to make, and repeat, observations in different parts of the globe, but, by causing this changeable quality to be better understood, may contribute to the benefit of navigation, and commerce,

* Phil. Trans. 1806.

commerce, as well as to the advancement of a more particular knowledge of the subject.

The magnetical variation is universally supposed to change in the course of time. Exc ption.

It has hitherto been considered, that the variation of the magnetical needle is not fixed in any particular place, but is constantly varying, in a greater or a less degree, in all parts of the world. I have discovered an exception to this supposed general property of variation; and, as it may be, perhaps, the first that has been made, it will require proportionally strong proof to establish it. This, I flatter myself, I am able to effect, to the certainty of demonstration itself; but, in doing so, I am under the necessity of being more tedious than I could wish, in order to describe fully the data, on which the inference is founded.

Lands in Jamaica are held under grants with a diagram annexed

I resided at Jamaica, as a King's Surveyor of Land, upwards of twenty years. Disputes at law about boundaries of lands are there decided by ejectments, in the Supreme Court of Judicature, by the evidence and diagrams of King's Surveyors of Land. This is different from the practice in England, because the manner in which grants of land from the Crown are made, in the two countries is different. In Jamaica, to every grant of land a diagram thereof is annexed to the patent. This diagram is delineated from an actual survey of the land to be granted, having a meridional line, according to the magnetical needle, by which the survey was made, laid down in it. No notice is taken of the true meridian. The boundary lines of the land granted are marked on earth, (as it is denominated,) by cutting notches on the trees between which the line is run through the woods.

— referring to permanent lines of the estate, determined at first by compass.

These trees being mostly of hard timber, the notches will be discernible for thirty years, or more. By repeated re-surveys these lines are kept up; and, when the cultivation, on both sides, renders it necessary to fell the marked trees, (which can only be done by mutual consent, it being otherwise death by the law,) logwood fences are planted in the lines dividing the properties thus cultivated; and many of these fences have been regularly repaired, and kept up, to the present time. Lands were granted from the Crown soon after the Restoration, in 1660; and every succeeding year the number of patents increased.

increased. The old estates have been often re-surveyed, and plans of them made, and usually annexed to deeds of conveyance, or mortgage, which must be enrolled, within a limited time, in the office of the Secretary of the Island! where, also, all the patents, and diagrams annexed to them, are recorded. In all disputes at law about boundary lines, where the keeping up of the old marked lines on earth has been neglected, surveyors are appointed to make actual re-surveys of all the old marked lines on earth, (preserved in the manner before mentioned,) and to extract from the Secretary of the Island's office correct copies of all such diagrams annexed to patents, and to deeds of conveyance, or mortgage, of lands in the neighbourhood where the disputed boundary is, as they may think necessary for the investigation thereof. They then compare the lines, and meridians, of these original diagrams with those in their diagrams delineated from their own re-surveys recently made; when it is always expected that the lines, and meridians, of the former will coincide with those of the latter. It is evident that this coincidence could not happen if any variation of the magnetical needle had taken place in the intermediate time elapsed between the making of the first and of the last survey. My business being very extensive, I was frequently applied to in disputes at law about boundary lines, and I had, besides, abundance of opportunities, on other surveys, to ascertain this fact satisfactorily. From all which I have discovered that the courses of the lines, and meridians, delineated on the original diagrams annexed to patents, from 1660, downwards to the present time, and of the re-survey diagrams thereof, annexed to deeds, coincide with, and are parallel to, the lines and meridians delineated on the new diagrams from recent surveys made by the magnetical needle, of the same original marked lines on earth, (preserved as before described); so that whatever course is laid down for the line on the diagram annexed to the patent, (and let it be supposed, for example, to be north and south, or east and west,) upon setting the compass in the old marked line on earth, and directing the sights north and south, or east and west, according to the magnetical needle, the said

The compass at Jamaica shews the same bearings— on the ground as were determined 130 or 140 years ago.

said marked line on earth, originally run by the magnetical needle 130 or 140 years ago, has been found by me to be exactly in the line, or direction with that of the compass; consequently no alteration of the variation could have taken place during the whole, or any part, of that period of time in Jamaica.

Qu. Whether the variation was allowed in ancient times?

To this it may not be unacceptable to subjoin a short history of the practice of surveying in Jamaica, from the Restoration to the present time, in order to obviate any doubt that might arise, whether there be not a possibility of the quantity of the magnetical variation having been ascertained, and allowed for, in the first diagrams annexed to patents: and whether the variation of 6¼ degrees east, which corresponds with the magnetical needle now, might not then, have agreed with the true meridian.

The variation first noticed by Columbus,

The variation of the compass was first observed by COLUMBUS, in his first voyage across the Atlantic, in the year 1492; and seemed to threaten that the laws of Nature were altered in an unknown ocean. It is evident, however, that Columbus was not able to ascertain the quantity of variation; for if he had ascertained it, the danger he was in would have been diminished, if not entirely removed. His discovery, therefore, must have been simply the deflection of the magnetical needle from the true meridian, without knowing the quantity thereof.

— and systematized by Halley, who in 1700 found the same variation at Jamaica as now.

From this period down to the year 1700, when Dr. Halley published his "Theory of the Variation of the Compass," no observations, ascertaining the quantity of variation, in the West Indies, were, I believe, published. He was the first that made any in South America, and these were chiefly applicable to the coast of Brasil. With his theory was published "A new and correct Chart of "the whole World, shewing the Variations of the Com"pass, &c. as they were found in 1700, by Direction of "Capt. Edm. Halley." By this chart the variation, at Jamaica, appears to have been the same as it is at present. His theory could have been known but to few; nor do any observations, in the West Indies, appear to have been made for many years after its publication. Indeed I know of none till very lately, and these only in a few charts. But, however extensive its publicity might have been,

been, it could have had no influence in directing the surveys, in Jamaica, that preceded it by 30 or 40 years.

The ascertaining of the true meridian, and, consequently, of the magnetical variation from it, requires more scientifical, as well as practical, knowledge, than is often to be met with even at this time; but, 130 or 140 years back, it was entirely confined to a very few individuals. The magnetical needle was then the only guide and rule to go by, both at sea and at land, and, generally, without any reference being had to the true meridian.

The early surveyors were guided only by the compass—

Had the first surveyors ascertained the quantity of variation, and allowed for it, in delineating their diagrams that were annexed to the earliest patents in Jamaica, they would have mentioned the same in such diagrams; otherwise it could only tend to mislead, not to direct. The same system of surveying would, and must, by law, have been continued; for, (as was stated above,) the number of grants has been annually increased; and the uninterrupted practice of surveying, which was always daily increasing in proportion to the extending cultivation and settlement of the island, could not admit of any change, without a new law having been made by the legislature for that purpose; and then such a change must have been recorded with the laws of the island, and with those that regulated the conduct of surveyors. No surveyor, nor other person, could have been ignorant of such a change having taken place. Since even the difference of one degree in running a line is very considerable; but that of six would have totally changed all property, deranged all boundaries, thrown woodlands into plantations, and *vice versâ*: and, consequently, would have been so palpable and injurious as to have demanded legislative interference and correction. But no such change has ever happened, nor has the most remote idea of it ever been entertained. On the contrary, the magnetical meridian, in all disputes at law about boundary lines, is, and always has been, the only criterion by which the surveyors, the court, and the jury, decide.

—and certainly allowed nothing for variation in their drafts.

If it had afterwards changed the legislature must have noticed it.

From the year 1700, when Dr. Halley's theory was published, it is very easy to trace down the practice of surveying in Jamaica, as well as up to its commencement. When

The early field books and documents of surveys have.

been preserved.

When I arrived in that island, upwards of twenty-five years since, I became acquainted with the oldest surveyors there, who had practised from thirty to forty years. They had the original papers, field notes, and diagrams of their predecessors, up to the dates of the first surveys. Many of these original papers, field notes, and diagrams are now in my possession; from which the practice of surveying, taking field notes, and delineating them on diagrams, is clearly shown.

The original division of Jamaica into counties and parishes was made by compass, and the compass still agrees with it.

Jamaica was early divided into counties and parishes, the boundary lines whereof were defined by the legislature, and the lines of many marked on earth. In the county of Surrey, the line, dividing the parishes of Portland and St. George, is a north and south line, by law, and was marked on earth according to the magnetical needle. It continues in the same direction. In the county of Cornwall, the dividing line between the parishes of St. James and Trelawney continues a north and south line, on earth, as it was first run by the magnetical needle. This will be evident on the inspection of my maps of Jamaica, lately published. It became necessary, in giving the island its true position on the globe, to ascertain its latitude and longitude; and also the true meridian, with the quantity of the variation of the magnetical meridian from it. But I have applied these meridians differently in the maps of the counties, and in that of the island. In the former, in which the situation on the globe is not given, the magnetical is laid down as the principal meridian; because all surveys of every other description, as well as those of the boundary lines of counties and parishes, are regulated by it; and the true meridian is introduced only to shew the variation; but, in the latter, in which its place on the globe is fixed, as to latitude and longitude, the true meridian becomes the principal one; and the magnetical meridian shows the quantity of variation from it, and regulates the surveys, and the relative situation of places, as in the county maps.

The surveys under Sir Henry Moore, in

When Sir Henry Moore, (who was considered a great surveyor,) was governor of Jamaica, about the year 1760, maps of that island were constructed, under his immediate

diate direction, by Mr. CRASKELL, the island engineer, and Mr. Simpson, both eminent surveyors. But, in these maps, the magnetical meridian only is represented. Neither the magnetical variation nor the true meridian is mentioned: the island's place on the globe, as to latitude and longitude, is not given. In short, the true meridian has never been noticed, nor the quantity of variation ascertained, nor the variation even mentioned, nor the latitude and longitude, observed by any *surveyor* or *engineer* in Jamaica, but myself. 1760, were made without notice of the variation.

Although the discovery of the variation's not varying, in Jamaica, is established on the clearest evidence without the aid of other data, yet it is highly gratifying to find Dr. Halley, as it were, confirming it to the minutest accuracy, as will appear from the recital of the following observations of Mr. Long, in his History of Jamaica.

" The variations of the magnetical needle were ob-Quotation
" served by Dr. Halley to be very small, near the equa- shewing the
" tor. I have seen no account of them for this island, variation in
" that can be relied upon; but, if observations should 1700 to have
" be faithfully made here, they would probably confirm been 6 deg. and
" his opinion. According to Mountain's chart, con- a half east—
" structed in the year 1700, from Dr. Halley's tables,
" the variation at Port Royal then was about $6\frac{1}{2}$ degrees
" east. But, as in most parts of the world it is found
" to be continually either increasing or decreasing, so
" we may reasonably conclude, that it may have altered
" in both respects very much during this long interval
" that has passed since the construction of the chart."

The magnetical variation, ascertained by me, and laid down in my maps of Jamaica, is $6\frac{1}{2}$ degrees east. —and it is the same now (1806).

I leave to others, better qualified than I am, to inquire and to point out, what improvements natural philosophy may derive from this discovery; which I hope may be an acquisition to science.

I am afraid I have been too prolix. But the importance of the subject, and my desire to remove even the shadow of any doubt that might be suggested, will, I trust, be admitted as my apology.

I have the honour to be, &c.

JAMES ROBERTSON.

VIII.

Observations and Remarks on the Figure, the Climate, and the Atmosphere of Saturn, and its Ring. By WILLIAM HERSCHEL, *LL. D. F.R.S**.

Changes in the appearance of Saturn—

MY last year's observations on the singular figure of Saturn having drawn the attention of astronomers to this subject, it may be easily supposed that a farther investigation of it will be necessary. We see this planet in the course of its revolution round the sun in so many various aspects, that the change occasioned by the different situations in which it is viewed, as far as relates to the ring, has long ago been noticed; and Huygens has given us a very full explanation of the cause of these changes †.

— affect the body of the planet as well as the ring.

As the axis of the planet's equator, as well as that of the ring, keeps its parallelism during the time of its revolution about the sun, it follows that the same change of situation, by which the ring is affected, must also produce similar alterations in the appearance of the planet; but since the shape of Saturn, though not strictly spherical, is very different from that of the ring, the changes occasioned by its different aspects will be so minute that only they can expect to perceive them who have been in the habit of seeing very small objects, and are furnished with instruments that will show them distinctly, with a very high and luminous magnifying power.

Jupiter is not so affected, because its equator is nearly in the ecliptic.

If the equator of the planet Jupiter were inclined to the ecliptic like that of Saturn, I have no doubt but that we should see a considerable change in its figure during the time of a synodical revolution; notwithstanding the spheroidical figure occasioned by the rotation on its axis has not the extended flattening of the polar regions that I have remarked in Saturn. But since not only the position of the Saturnian equator is such that it brings on a periodical change in its aspect, amounting to more than

* Philos. Trans. 1806.

† See *Systema Saturnium*, page 55, where the changes of the ring are represented by a plate.

62 degrees

62 degrees in the course of each revolution, but that moreover in the shape of this planet there is an additional deviation from the usual spheroidical figure arising from the attraction of the ring, we may reasonably expect that our present telescopes will enable us to observe a visible alteration in its appearance, especially as our attention is now drawn to this circumstance.

Equatorial and polar diam. of Saturn, &c.

In the year 1789 I ascertained the proportion of the equatorial to the polar diameter of Saturn to be 22,81 to 20,61 *, and in this measure was undoubtedly included the effect of the ring on the figure of the planet, though its influence had not been investigated by direct observation. The rotation of the planet was determined afterwards by changes observed in the configuration of the belts, and proper figures to represent the different situation of the spots in these belts were delineated †. In drawing them it was understood that the shape of the planet was not the subject of my consideration, and that consequently a circular disk, which may be described without trouble, would be sufficient to show the configurations of the changeable belts.

Figure of Saturn.

Those who compare these figures, and others I have occasionally given, in which the particular shape of the body of the planet was not intended to be represented, with the figure which is contained in my last paper, of which the sole purpose was to express that figure, and wonder at the great difference, have probably not read the measures I have given of the equatorial and polar diameters of this planet; and as it may be some satisfaction to compare the appearance of Saturn in 1789 with the critical examination of it in 1805, I have now drawn them from the two papers which treat of the subject; Fig. 3. Plate I. represents the spheroidical form of the planet as observed in 1789, at which time the singularity of the shape since discovered was unknown; and Fig. 4. represents the same as it appeared the 5th of May 1805. The equatorial and polar diameters that were established in 1789 are strictly preserved in both figures, and the last differs from the first only in having the flattening at the

* Phil. Trans. for 1790, p. 17.

† Ibid. for 1792, p. 22.

 poles

poles a little more extended on both sides toward the equatorial parts. It is in consequence of the increase of the length of this flattening, or from some other cause, that a somewhat greater curvature in the latitudes of 40 or 45 degrees north and south has taken place; and as these differences are very minute, it will not appear extraordinary that they should have been overlooked in 1789, when my attention was entirely taken up with an examination of the two principal diameters of the planet.

On magnifying powers, particularly low powers.

The use of various magnifying powers in observing minute objects is not generally understood. A low power, such as 200 or 160, with which I have seen the figure of Saturn, is not sufficient to show it to one who has not already seen it perfectly well with an adequate high power; an observer, therefore, who has not an instrument that will bear a very distinct magnifying power of 500, ought not to expect to see the outlines of Saturn so sharp and well defined as to have a right conception of its figure. The quintuple belt is generally a very good criterion: for if that cannot be seen, the telescope is not sufficient for the purpose; but when we have entirely convinced ourselves of the reality of the phenomena I have pointed out, we may then gradually lower the power, in order to be assured that the great curvature of the eye-glasses giving these high powers, has not occasioned any deceptions in the figure to be investigated, and this was the only reason why I mentioned that I had also seen the remarkable figure of Saturn with low powers.

The figure of Saturn was not affected by gibbosity.

In very critical cases it becomes necessary to calculate every cause of an appearance that falls under the province of mathematical investigation. For this reason I have always looked upon an astronomical observation without a date as imperfect, and the journal-method of communicating them is undoubtedly what ought to be used. For instance, when it is known that my last year's most decisive observation, relating to the singular figure of Saturn, was made the 5th of May, astronomers may then calculate by this date the place of Saturn and of the earth; their distances from each other, and the angle of illumination of the Saturnian disk; by these means we

we find the gibbosity of the planet in the given situation, and ascertain that the defalcation of light could not then amount to the one-hundredth part of a second of a degree, and that consequently no error could arise from that cause.

I have divided the following observations into two heads, one relating entirely to the figure of the body of Saturn, the other concerning the physical condition or climate and atmosphere of the planet.

Observations of the Figure of Saturn.

In the collection of my observations on the planet Saturn, I have met with one made eighteen years ago, which is perfectly applicable to the present subject, and is as follows:

Observations on the figure of the planet. Suspicion in 1788 that Saturn is not spheroidal.

August 2, 1788, 21^h 58′. 20-feet reflector, power 300. Admitting the equatorial diameter of Saturn to lie in the direction of the ring, the planet is evidently flattened at the poles. I have often before, and again this evening, supposed the shape of Saturn not to be spheroidical, (like that of Mars and Jupiter,) but much flattened at the poles, and also a very little flattened at the equator, but this wants more exact observations.

Observations shewing the apparent figure of Saturn and the variations it is subject to.

April 16, 1806. I examined the figure of the body of Saturn with the 7 and 10-feet telescopes, but they acted very indifferently, and, were I to judge by present appearances, I should suppose the planet to have undergone a considerable change; should this be the case, it will then be necessary to trace out the cause of such alterations.

April 19. 10-feet, power 300. The polar regions are much flattened. The figure of the planet differs a little from what it appeared last year. This may be owing to the increased opening of the ring, which in four places obstructs now the view of the curvature in a higher latitude than it did last year. The equatorial regions on the contrary are more exposed to view than they have been for some time past.

May 2. 10-feet, power 375. The polar regions are much flatter than the equatorial; the latter being more disengaged from the ring appear rather more curved than last

Observations shewing the apparent figure of Saturn and the variations it is subject to.

last year, so that the figure of the planet seems to have undergone some small alteration, which may be easily accounted for from our viewing it now in a different aspect.

The planet Jupiter not being visible, we cannot compare the figure of Saturn with it; but from memory I am quite certain that the flattening of the Saturnian polar regions is considerably more extended than those of Jupiter.

May 4. 10-feet, power 527. The equatorial region of Saturn appears to be a little more elevated than last year. This part of the Saturnian figure could not be examined so well then as it may at present, the ring interfering with our view of it in four places, which are now visible.

The flattening on both sides of the pole is continued to a greater extent than in a figure merely spheroidical, such as that of Jupiter; and this makes the planet more curved in high latitudes.

The planet being in the meridian, the equatorial shape of Saturn appears a little more curved than last year; but the air is not sufficiently pure to bear high powers well.

May 5. 10-feet, power 527. The air is very favourable, and I see the planet well with this power; its figure is very little different from what it was last year.

The polar regions are more extendedly flat than I suppose they would have been if the planet had received its form only from the effect of the centrifugal force arising from its rotatory motion.

The equatorial region is a little more elevated than it appeared last year.

The diameter which intersects the equator in an angle of about 40 or 45 degrees is apparently a little longer than the equatorial, and the curvature is greatest in that latitude.

The planet being in the meridian and the night beautiful, I have had a complete view of its figure. It has undergone no change since last year, [illegible] arises from its different situation, and a gr[illegible] the ring.

May 9. Power 527. The air being very clear, I see the figure of Saturn nearly the same as last year; the flattening at the poles appears at present somewhat less; the equatorial and other regions are still the same.

Observations shewing the apparent figure of Saturn and the variations it is subject to.

May 15, 10^h 30′. I examined the appearance of Saturn, and compared it with the engraving representing its figure in last year's volume of the Phil. Trans. The outlines and all the other features of this engraving are far more distinct than we can ever see them in the telescope at one view; but it is the very intention of a copper-plate to collect together all that has been successfully discovered by repeated and occasional perfect glimpses, and to represent it united and distinctly to our inspection. Indeed by looking at the drawings contained in books of astronomy this will be found to be the case with them all*.

The equatorial diameter of my last year's figure is however a very little too short; it should have been to the polar diameter as 35,41 to 32, which is the proportion that was ascertained in 1789, from which I have hitherto fonnd no reason to depart.

The following particulars remain as my last year's observations have established them.

The flattening at the poles of Saturn is more extensive than it is on the planet Jupiter. The curvature in high latitudes is also greater than on that planet. At tho equator, on the contrary, the curvature is rather less than it is on Jupiter.

Upon the whole, therefore, the shape of the globe of Saturn is not such as a rotatory motion alone could have given it.

I see the quintuple belt, the division of the ring, a very narrow shadow of the ring across the body, and another broader shadow of the body upon the following part of the ring; and unless all these particulars are very distinctly visible we cannot expect that our instrument

* For an instance of this, see TOBIÆ MAYERI *Opera inedita. Appendix Observationum. Ad Tabulam Selenographicam Animadversiones*, where the annexed accurate and valuable plate represents the moon such as it never can be seen in a telescope.

should

Observations shewing the apparent figure of Saturn and the variations it is subject to.

should show the outlines of the planet sufficiently well to perceive its peculiar formation.

May 16, 10^h 10'. The greatest curvature on the disk of Saturn seems to be in a latitude of about 40 degrees.

May 18. The difference between the equatorial and polar diameters appears to be a little less than the measures taken September 14, 1789, give it; but as the eye was then in the plane of the equator, and is now about 16 degrees elevated above it, we cannot expect to see it quite so much flattened at present.

June 3. The shadow of the ring falls upon the body of the planet southwards of the ring, toward the limb; it grows a little broader at both ends where it is upon the turn round the globe.

June 5. The planet Jupiter is not sufficiently high for distinct vision, and Saturn is already too low to use a proper magnifying power; but nevertheless the difference in the formation of the two planets is evident. The equatorial as well as polar regions on Jupiter are more curved than those of Saturn.

June 9. The air is beautifully clear, and proper for critical observations.

The breadth of the ring is to the space between the ring and the body of Saturn as about 5 to 4. See Fig. 3.

The ring appears to be sloping toward the body of the planet, and the inside edge of it is probably of a spherical or perhaps hyperbolical form.

The shadow of the ring on the planet is broader on both sides than in the middle; this is partly a consequence of the curvature of the ring which in the middle of its passage across the body hides more of the shadow in that place than at the sides.

The shadow of the body upon the ring is a little broader at the north than the south, so as not to be parallel with the outline of the body; nor is it so broad at the north as to become square with the direction of the ring.

The most northern dusky belt comes northwards on both sides as far as the middle of the breadth of the ring where it passes behind the body. It is curved toward the south in the middle.

I viewed

I viewed Jupiter, and compared its figure with that of Saturn. An evident difference in the formation of the two planets is visible. To distinguish the figure of Jupiter properly it may be called an ellipsoid, and that of Saturn a spheroid.

Observations on the periodical Changes of the Colour of the polar Regions of Saturn.

Probability that the polar regions of Saturn are frozen.

In the observations I have given on the planet Mars, it has been shown that an alternate periodical change takes place in the extent and brightness of the north and south polar spots*; and I have there suggested an idea that the cause of the brightness might be a vivid reflection of light from frozen regions, and that the reduction of the spots might be ascribed to their being exposed to the sun.

The following observations, I believe, will either lead us to similar conclusions with respect to the appearance of the polar regions of Saturn, or will at least draw the attention of future observers to a farther investigation of the subject.

Useful application of high powers to determine the relative brightness of objects.

With high magnifying powers, the objects we observe require more light than when the power is lower; this affords us a good method of determining the relative brightness of the different parts of a planet. The less bright object will be found deficient in illumination when the power exceeds what it will bear with ease. I have availed myself of this assistance in the observations that follow.

Observations on the appearance of Saturn and its ring, partial changes of brightness, &c.

June 25, 1781. With an aperture of 6,3 inches I used a magnifying power of 460. This gave a kind of yellowish colour to the planet Saturn, while the ring still retained its full white illumination.

November 11, 1793. From the quintuple belt toward the south pole the whole distance is of a pale whitish colour; less bright than the white belts, and much less bright than the ring.

This has been represented in a figure which was given in the volume of the Phil. Trans. for 1794, page 32. It

* Phil. Trans. for 1784, page 260.

is to be noticed that the south pole of the planet had been long exposed to the influence of the sun, and the former polar whitishness was no longer to be seen.

Jan. 1, 1794. The south polar regions are a little less bright than the equatorial belt.

Nov. 5, 1796. The space between the quintuple belt and the northern part of the ring is of a bright white colour.

This seems to indicate that the whiteness of the northern hemisphere of Saturn increases when there is less illumination from the sun.

May 6, 1806. The north pole of Saturn being now exposed to the sun, its regions have lost much of their brightness; the space about the south pole has regained its former colour, and is brighter and whiter than the equatorial parts.

May 15. The south polar regions of Saturn are white; those of the north retain also some whitishness still.

May 18. With a magnifying power of 527, the south polar regions remain very white. The equatorial parts become of a yellowish tinge, and about the north pole there is still a faint dusky white colour to be seen.

June 3. The south polar regions are considerably brighter than those of the north.

The changes correspond with winter and summer on the planet.

These observations contrasted with those which were made when the south pole was in view complete nearly half a Saturnian year, and the gradual change of the colour of the polar regions seems to be in a great measure ascertained. Should this be still more confirmed, there will then be some foundation for admitting these changes to be the consequence of an alteration of the temperature in the Saturnian climates. And if we do not ascribe the whiteness of the poles in their winter seasons immediately to frost or snow, we may at least attribute the different appearance to the greater suspension of vapours in clouds, which, it is well known, reflect more light than a clear atmosphere, through which the opaque body of the planet is more visible. The regularity of the alternate changes at the poles ought however to be observed for at least two or three of the Saturnian years, and this, on account of their extraordinary length, can only

only be expected from the successive attention of astronomers.

On the Atmosphere of Saturn.

Atmosphere of Saturn and of its ring.

June 9, 1806. The brightness which remains on the north polar regions, is not uniform, but is here and there tinged with large dusky looking spaces of a cloudy atmospheric appearance.

From this and the foregoing observations on the change of the colour at the polar regions of Saturn arising most probably from a periodical alteration of temperature, we may infer thee existence of a Saturnian atmosphere; as certainly we cannot ascribe such frequent changes to alterations of the surface of the planet itself; and if we add to this consideration the changes I have observed in the appearance of the belts, or even the belts themselves, we can hardly require a greater confirmation of the existence of such an atmosphere.

A probability that the ring of Saturn has also its atmosphere has already been pointed out in a former Paper.

Slough, near Windsor,
June 12, 1806.

SCIENTIFIC NEWS.

A Report of the Transactions of the Class of Mathematical and Physical Sciences of the National Institute of France, for the preceding Year, was made at the public Meeting of the 7th of July last, of which the following is an abridgment.

(Concluded from p. 337.)

Geographical researches.

TO these geometrical considerations respecting the figure of the earth the reporter observes, that the order of connection would naturally direct him to the geographical researches on the extensive plain of the interior of Africa, by Lacepede; upon Persia, and the communication between the Caspian and the Black Seas, by Olivier; but as these memoirs more particularly belong to physical science, and as such have been analized by Cuivier, he passes to the consideration of M. Raymond's memoir upon the Admeasurement of the Heights of Mountains by means of the Barometer.

Measure of heights of mountains by the barometer.

It was remarked in the Report of 1805, that there was scarcely one five-hundredth part of difference between the coefficient of La Place for calculating the heights of mountains by barometrical observation and that which M. Raymond has deduced from numerous observations of this kind made in the Pyrennees. New researches have entirely obliterated a difference which might be attributed either to the barometrical observations or to the earlier experiments on the respective weights of air and mercury which M. La Place had used in his computations. M. Biot has lately repeated these experiments with the utmost precaution; from whence it results that the coefficient must be diminished very nearly one five-hundredth, and the methods perfectly agree together.

On

On the one hand we observe the geometer assuming as data the facts observed in the cabinet of an experimental philosopher deduces a rule for measuring the heights of mountains; and on the other, an observer assuming for his basis of deduction the known height of a mountain, and the effect which it produces upon the elevation of the mercurial column in the barometer, draws his conclusion as to the relative weights of mercury and of air, and finds the same quantities which were made use of by the geometer for establishing his calculations. These comparisons, which become every day more numerous in the application of analysis; these identical results obtained by processes so contrary, and deduced from phenomena so different, are proofs which establish the sciences beyond all question.

Circumstances necessary to be attended to in barometrical observations.

This important result does not constitute the only merit of M. Raymond's memoir. Methods of distinguishing the circumstances most favourable or most inimical to this description of observations, are pointed out and arranged under three different titles,—The influence of the time of day, of the stations, and of the meteors. As to the time of day, it is found that the heights observed in the morning and the evening are always too small; whence it follows that observations ought always to be made about the middle of the day, which is a condition very easy to be complied with. The influence of stations is not less real, but more difficult to be obviated. The rule to be followed is, that the portable barometer and the barometer of comparison should be as nearly as possible in stations where the local circumstances are the same. A great distance or interval is not always an obstacle; so that M. Raymond has remarked that observations made by him on the Pyreneau Mountains, when compared with those which M. Bouvard continually makes at the Impe ial Observatory, present a course of changes of considerable regularity, whereas the same observations of M. Bouvard, compared with those which M. Raymond made at Marli la Ville, indicate from one day to another, differences of ten or twelve metres or yards in the relative height of the two stations: whence it may be concluded, that the use of the barometer to measure

measure heights not much differing from each other has not much certainty when the two stations are on a plane.

With regard to the influence of meteors, it always acts in the same direction, and causes the heights to appear too small; whence all observations are to be rejected which were made in stormy weather. From all these considerations it follows that in order to have the most exact height of a mountain it will not be proper to take a mean indifferently between all the observations made at different hours and seasons, as in this case the elevation would always prove less than the truth.

Various researches, discoveries, &c.

An extensive course of experiments by M.M. Biot and Arago upon the affinities between the different gases and light are not spoken of by the reporter, because M. Biot purposes to give an extract himself. An abridgment of Count Rumford's memoir on the dispersion of the light of lamps through ground glass, likewise composes part of the present report, which need not be given in this place, because the Count's memoir has already appeared in our Journal.

Simple notices of inventions in Science and the Arts which are entitled to honourable mention, are likewise given in the present report, but as they contain no accounts of the methods or processes, it has not been thought necessary to repeat them here. Since its last public sitting the Class has published the first volume of Memoirs presented by learned Foreigners (Savans Etrangers), and the sixth volume of its own Memoirs. The following volumes will be published every six months, beginning with July last. The first volume of La Méridienne de Dunkerque, base du Système Mêtrique décimal. This work contains all the authorities, observations, and methods of calculation which have fixed the two fundamental unities of the Metrical System, namely, the Metre and the Kilogramme.

Various publications.

Several members have published new works, or new editions of works already known, in which important additions are found. Among these M. Legendre has published a sixth edition of his Geometry, and Lacroix a second

second edition of his Elementary Treatise on the differential and integral Calculus. Various publications.

The astronomical world is now in possession of Solar Tables, in the computation of which the attractions of all the planets have, for the first time, been admitted.

Lastly, Le Grange has given a more complete edition of his Calcul des Fonctions, a truly classical work, which requires no recommendation to those mathematicians who have perused it, and could with no degree of facility be properly announced to those who have not. La Place has also published a Dissertation, forming a supplement to the tenth book of his Mecanique Celeste, in which he gives a complete theory of the capillary action of which some notice has before been taken in our Journal. These delicate researches will not be considered merely as trials of skill by those who are aware how extensively the phenomena of Nature are connected with each other. Every variable quantity among natural appearances becomes the measure for determining other results as soon as the law of its variation is ascertained. One object of utility is pointed out by La Place with respect to measuring heights by the barometer, in which a question has arisen, Whether the length of the mercurial column should be determined from the base or the summit of its convexity. Our author shews, that the latter is much the most correct, though this height is less than would be produced by the atmospheric pressure if the capillary repulsion did not act. He gives two methods of correction.

Philosophical Transactions of the Royal Society of London for the Year 1806, Part II. 4to. 454 pages, with 12 Plates. London. Nicol. Royal Society.

This part contains the following articles: 1. Observations upon the Marine Barometer, made during the examination of the Coasts of New Holland and New South Wales, in the Years 1801, 1802, and 1803. By Matthew Flinders, Esq. Commander of his Majesty's ship Investigator. 2. Account of a Discovery of native Minium. By James Smithson, Esq. F.R.S. 3. Description of a rare Species of Worm Shells discovered at an Island lying off the North-west Coast of the Island of Sumatra, in the East Indies. By J. Griffiths, Esq.

4. Observations

Various publications.

4. Observations on tho Shell of the Sea Worm found on the Coast of Sumatra, proving it to belong to a Species of Teredo; with an Anatomy of the Teredo Navalis. By Everard Home, Esq. F. R. S. 5. On the inverted Action of the alburnous Vessels of Trees. By A. Knight, Esq. F.R.S. 6. A new Demonstration of the Binomial Theorem, when the Exponent is a positive or negative Fraction. By the Rev. Abram Robertson, A.M. F.R.S. Savilian Professor of Geometry in the University of Oxford. 7. New Method of computing Logarithms. By Thomas Manning, Esq. 8. Description of the mineral Bason in the Counties of Monmouth, Glamorgan, Brecon, Carmarthen, and Pembroke. By Mr. Edward Martin. 9. Observations on the Permanency of the Variation of the Compass at Jamaica. By Mr. James Robertson. 10. Observations on the Camel's Stomach, respecting the Water it contains, and the Reservoirs in which that Fluid is enclosed, &c. By Everard Home, Esq. F.R.S. 11. Observations on the Variation and on the Dip of the Magnetic Needle between 1786 and 1805 inclusive. By Mr. George Gilpin. 12. On the Declinations of some of the principal fixed Stars, with a Description of an Astronomical Circle, and some Remarks on the Construction of circular Instruments. By John Pond, Esq. 12. Observations and Remarks on the Figure, the Climate, and the Atmosphere of Saturn and its Ring. By William Herschell, LL. D. F.R.S. &c.

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A
JOURNAL
OF
NATURAL PHILOSOPHY,
CHEMISTRY,
AND
THE ARTS.

VOL. XVI.

Illustrated with Engravings.

BY WILLIAM NICHOLSON.

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1807.

PREFACE.

THE Authors of Original Papers and Communications in the present Volume are, G. Cumberland, Esq.; X.; Mr. John Tatum, Jun.; Mr. John Webster; Mr. W. Skrimshire, Jun.; G. C.; John Bostock, M. D.; Mr. J. Hume; R. B.; Mr. Knox; T. Thomson, M. D. F. R. S. E.; Dr. Halliday; Sir H. C. Englefield, Bart. M. P. F. R. S.; David Brewster, M. A.; A.; A Constant Reader.

Of Foreign Works, Dr. Carradori; M. Montgolfier; M. Bouillon Lagrange; M. Ch. Hersart; Professor Proust; M. De Lalande; Samuel Mitchell.

And of British Memoirs abridged or extracted, Mr. John Gough; Mr. William Watson; Dr. J. A. Hamilton, Dean of Cloyne; Rev. James Little; Thomas Andrew Knight, Esq. F. R. S.; Matthew Flinders. Esq.; Mr. W. Hardy; Mr. Andrew Flint; James Smithson, Esq. F. R. S.; G. Mitchell, M. B.; Patrick Neill, A. M.; Mr. Peter Herbert; Mr. John Antis; J. C. Curwen, Esq. M. P.; John Pond, Esq.; Sir John Sinclair, Bart. M. P.; Dr. William Roxburgh; Mr. H. Steinhauer; Rev. Peter Roberts, A. M.; Dr. Cogan; Rev. William Richardson; Mr. George Gilpin; Mr. S. Grandi; Mr. Neill Snodgrass; Thomas Egan, M. D. F. R. S.; William Alexander, M. D.; John Alderson, M. D.; Rev. Gilbert Ausein, M. R. I. A.; Mr. Edward Martin.

Of the Engravings the Subjects are, 1. A Diagram by Mr. John Gough, to illustrate the Doctrine of mixed Gases; 2. A very simple Scale for Drawing the Vanishing Lines in Perspective, by G. Cumberland, Esq.; 3. Figures by Dr. Herschell exhibiting the singular Outline of the Disk of Saturn; 4. Three Figures exhibiting the Acoustic Experiment of the Invisible Girl: 5. An expanding Band Wheel for driving Machinery, by Mr. Flint: 6. A new Door Latch, by Mr. Antis: 7. An improxed Book Case Bolt, by Mr. Peter Herbert: 8 Mr. Hardy's Compensation Balance for a Time Keeper: 9. A Calorimeter for measuring the relative Effects of Fuel, by M. Montgolfier: 10. Apparatus used in the Manufactory of forged Iron Vessels: 11. An Astronomical Circle, by Mr. Troughton: 12. A Drag for raising the Bodies of drowning Persons, by Dr. Cogan: 13. Sir H. C. Englefield's Method of adjusting the Transit Instrument: 14. A new Astrometer, by David Brewster, A. M.; 15. Mr. Snodgrass's Methods for heating Rooms by Steam; 16. Apparatus for transferring Gases over Water, Mercury,

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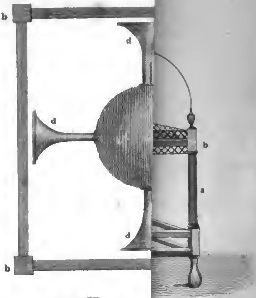
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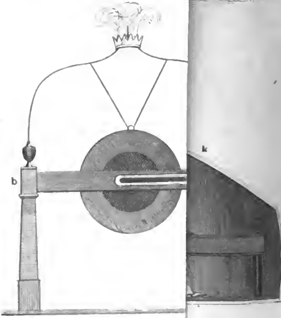
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A
JOURNAL
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THE ARTS.

JANUARY, 1807.

ARTICLE I.

Description of a very simple and useful Scale, for dividing the Vanishing Lines in Perspective. In a Letter from G. CUMBERLAND, *Esq.*

To Mr. NICHOLSON.

SIR,

A VARIETY of occupations and speculations have of late forced me to neglect some former engagements, to use my poor endeavours in promoting the laudable ends of your truly interesting publication. Introduction.

But that I may not be thought to have entirely forgot them, I send you the following trifle, which, however simple the idea, will, I am sure, be the more valued by you on that very acaccount, if I have justly estimated the noble simplicity of your intelligent mind.

Having been in the habit of drawing for my amusement all my life, and feeling the value of that acquirement, it has been my practice to recommend to others as much of that acquisition as can with very little trouble be attained; I mean the putting into perspective common objects; such as simple landscapes, machines, buildings, and the interior of apartments, manufactories, &c. And where I have had an opportunity to The valuable art of Perspective.

May be very easily acquired

give four or five close lessons, I have generally seen my end obtained to their great satisfaction, without ever shewing them the *Jesuits*, or any other voluminous treatise; books that have hindered more from the study of art, than they have ever made artists; for a moment's consideration on this subject will convince any mind, capable of reflection, that, to accomplish the general ends that even most painters have in view with respect to that art, it is only necessary to know the use of *the points of sight and horizontal line*. For while men have agreed to avoid bevel lines in all their constructions that are intended for use or habitation, we shall only want as much knowledge of the art as will enable us to put these into perspective, and to assit us at first, before, by practice, we have attained a correct eye; for practice, daily practice, will soon do all the rest, even by barely drawing the interior of a large apartment or gallery, with the objects continually before us in common use.

He who can put horizontal and perpendicular lines into perspective, will soon be able to do every thing else.

Simple contrivance for Vanishing-Lines.

To save time, however, and to imprint the few lessons necessary to be given on the mind of a learner, I have, some time back, made use of the following simple contrivance, which I now send to you, as the most likely means of universally promoting this necessary preliminary study, where the first general principles have been instilled:—Take a sheet of paper of an octavo size, and rule it with very black ink, from A to B (Fig. 1, Plate I). This represents the horizontal line; then fix a point in the centre, at C; this we will call the moveable point of sight: afterwards cross it, as in the plate, with as many diagonal lines as you please; and thus you have an instrument prepared that will be a sure guide to an inexperienced eye, in taking the perspective lines of all objects placed at right angles; such as streets, buildings, churches, apartments, &c. by mearly placing it under the leaf you mean to draw them on from nature, so as to see them faintly through, as boys do their writing-copies, when young and inexperienced.

It is a ruled paper, having the lines thereon.

A corresponding plate of glass having similar lines.

But, to make this instrument more complete, we should add a plate of glass of the same size as the leaf of the drawing-book, on which the like dark lines should be drawn so as, by holding it up perpendicularly, we may see, and, as it were, render tangible, the truth of perspective lines of buildings; and for those whose sight is bad, or for very young people, it would not be amiss to take a copper-plate of the like dimensions, and with a fine needle gently scratch out the like lines, in which

—or a copper-plate, or printing print lines.

case there will be no necessity to take off the burrs, as the engravers call the ridges raised in ploughing copper; and, from this plate, *ten thousand* impressions may be taken of the faint lines, by way of guide, on the drawing-book of a young beginner, without injuring the plate; for I can assure your readers, that it is more difficult to erase a slight scratch from a sharp needle on copper, by the act of taking impressions, than the deepest cut of the graver; the reason of which is, that the ridges of the skin of the printer's hand can never enter that fine line, whereas, in a coarse one, he polishes the edges of it down by every operation, and thus renders it a smooth channel, at last undefined, and incapable of retaining the printing ink; and the reason I am so diffuse on the subject is, that I think the knowledge of it may be generally useful, particularly to those who wish to extend the publication of botanical outlines: as it is not necessary to be taught the art of engraving for those who can draw lines, to design on copper the peculiarities of plants, or their anatomy. How to trace deeper lines with certainty on copper as easily as on paper, I will have the pleasure to communicate to you at my next leisure moment.

Utility of engraving with the dry needle.

— particularly in botany.

But, to return to our subject,

To this simple contrivance, we may add a sheet of perpendicular lines, by which means the uprights will all be shewn; and for very heavy intellects, at first even the horizontal scale might be useful, though I never found it so among my acquaintance. There are also many little helps of simple contrivances to further the first acquirement of this plain branch of the art; that, if you approve the idea, I shall with pleasure transfer from my portfolio: but with respect to the application of this already described, it will be necessary to permise, that the scale should be longer than the drawing-book each way; by which means, by barely sliding it to the right or left, you can at pleasure place your point of sight more or less to the right, or left, or middle of the horizon; and, to be prepared for all circumstances, it would be as well to be provided also with a scale having a high horizon, and another with a very low one, such as the Dutch painters generally used, and which ever produces a picturesque effect, by giving many profiles of the elevations, and multiplying the lines of light.

Sheet for perpendicular lines.

Thus you have an easy expedient for a first help—practice will accomplish the rest; for we all know, or should know,

This easy expedient and practice will complete the artist.

that daily practice discloses to the industrious draftsman all the arcana of optical, aereal, and linear perspective, destitute, it is true, of terms to describe his acquirement; but to his own mind a perfectly intelligible and useful rule, by the help of which he can, with certainty, imitate all he sees on the theatre of the universe.

With respect and esteem,
I am, Sir,
Your obliged humble Servant,
Bristol, Dec. 4. 1806. G. CUMBERLAND.

An Essay on the Theory of mixed Gases, and the State of Water in the Atmosphere. By Mr. John Gough. *Communicated by Dr.* Holme.

The Essays of Mr. Dalton adverted to.

FOUR essays appear in the fifth volume of the Memoirs of the Literary and Philosophical Society of Manchester, which contain many new ideas relating to the constitution of mixed gases, and the sate of water in the atmosphere. The design of these papers is evidently intended to remove certain difficulties which must strike every man of science, who happens to peruse M. de Luc's Theory of atmospherical Vapour. This attempt has the double recommendation of ingenuity and novelty; but the leading opinions of the system, even in its present form, are liable to several objections, which I am going to point out, being generously invited to undertake the task, by the author himself. My doubts relative to the subject arise partly from mathematical considerations, and in part from the evidence of experiment. Certain objections of the first class dispose me to conclude, that an atmosphere constructed on Mr. Dalton's plan, will appear upon examination to be repugnant to the principles of the mechanical phylosophy; and a direct appeal to experiment has moreover convinced me, that well established facts contradict the essential points of the theory.

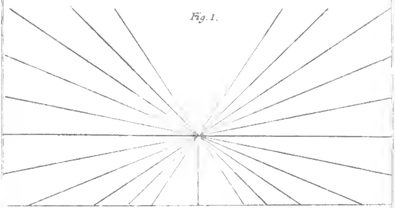
Concise view of Mr. Dalton's Theory of mixed gases.

To begin with the objections of the former class: I am ready to admit the existence of a fluid mixture, such as we find described at page 543, in the* fifth volume of the Manchester

* See our Journal, Vol. VI, p. 257.

Scale for the vanishing Lines of Perspective
by Geo Cumberland Esq.

Fig. 1.



Theory of mixed Gases
by John Gough Esq.

Fig. 4.

Fig 3

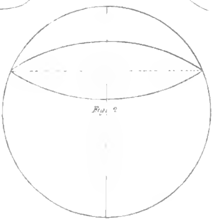


Fig. 2

Memoirs, with this reservation, that the concession is made, merely for the purpose of shewing such a combination to be incompatible with the usal course of things, for a moment; which being demonstrated, the inutility of the fundamental hypothesis will follow, as a necessary consequence.—To give a concise view of Mr. Dalton's general notion of the subject, we are to suppose a number of distinct gases to be confined in a space common to them all; which space may be circumscribed by the concave surface of a vessel, or the compressing power of an external fluid: besides this, we must imagine the constituent particles of each individuel gas to be actuated by a mutual repulsion, while, at the same time, they remain perfectly indifferent to the particles which compose the other fluids that are confined in the common space; in short, we are to conceive, that the particles of each gas act upon those of their own kind in the manner of elastic bodies; but that they obey the laws of inelastic bodies, as often as they interfere with corpuscles of a different denomination. After premising the preceding particulars, we may conceive a certain arrangement of the elementary parts of a fluid mixture, in which the adjustement of the whole shall be of a description which will form, from particles of any one denomination, a homogeneous fluid, possessing its own separate equilibrium; consequently, each gas will exist as an independant being, and exercise the functions of its elasticity, just as if all the other fluids were withdrawn from the common space. This systematic arrangement in an assemblage of gaseous substances cannot be maintained, unless one particular method of disposing its component parts be observed; wich consists in that distribution of the elements which will produce a separate equilibrium in the fluid composed by the elementary corpuscles of each denomination, consequently, the equilibrium in question cannot take place unles the necessary disposition of the heterogeneous particles be first established; so that the former requisite of the theory is entirely depended on the latter.—After having acquired a distinct idea of a fluid mixture, composed of gases possessing separate equilibria, we come in the next place to investigate the mechanical properties of such a compound; in the prosecution of which enquiry, the comparative densities of the constituent fluids must be first determined in a horizontal plane, the situation of which is given in the common space.

Examination of Mr. Dalton's theory of mixed gases.

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Let the figure P M I N K V, Plate 1, Fig. 2, represent this space, in which M V N K is the given plane.—Now since every point of this plane may be supposed to be at an equal distance from the earth's centre, the density of every homogeneous gas supported by it, will be the same in all parts of it. Let the constituent fluids be denominated A and B; also let C denote the compound; moreover let the densities of A and B, at P, be p and q; let P X and X Y be two equal evanescent parts of the line P V. Now seeing the pressure acting upon an elastic fluid is as the density of it, the fluxionarie increments of p and q, are as these quantities; but the densities of A and B, in the point X, are equal to the sums of p and q united to their increments respectively; let these sums be called e and f; then e is to f as p is to q, by composition of proportion: in like manner we find the density of A at Y to be to that of B at the same point as e is to f; i. e. as p is to q; thence it follows that the fluxionary increments of the two densities have universally the givin ratio of p to q; consequently the contemporary fluents, or the densities themselves have the same given ratio: now what has been proved of the two gases A and B may be extended to any other number; viz. the ratios of their densities, on the same horizontal plane will be given.

The ratio of A B, &c. being found to be constant, we can proceed to investigate the proportions of the quantities of matter contained in these fluids. Let D and d be the densities of A and B, in the plane MKNV; also let W and w be the quantities of matter of each kind, contained in the variable space PMKNV; call PV x, and the area of the plane MKNV y: now the fluxion of the space PMKNV is exprssed by y into the fluxion of x; moreover, the quantities of matter in two solids are in the complicate ratios of their magnitudes and densities, or in that of their densities only, if their magnitudes be equal; therefore the fluxion of W is to that of w as D is to d; because the fluxionary magnitude is common both to W and w; but D is to d as p to q, a constant ratio; consequently fluxion of W is to fluxion of w as p is to q; therefore W has to w the same given ratio; that is, the matter in A is to the matter in B as p is to q. In the next place, let R and r be the distances of the centres of gravity of A and B, from the point P, taken in the line PI; then R into the fluxion of W is equal to the product of D, Y, x, and the fluxion of x, from a well

known theorem in mechanics; for the same reason, r into the fluxion of w is equal to the product of d, y, x and fluxion x; hence R into fluxion of W is to r into fluxion of w, as D is to d; but D is to d, as fluxion W is to fluxion w; therefore R and r are equal: consequently the centres of gravity of A and B coincide, and the point of their coincidence is also the centre of the sytem C. Thus it appears, that when the component gases of a fluid mixture possess separate equilibria, their densities are every where in a given ratio; and they have a common centre of gravity; the converse of which is equally true, viz. if their densities be not every where in a given ratio, and if they have not a common centre of gravity, they do not possess separate equilibria.

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It is necessary to observe, in this stage of the inquiry, that though we admit the particles of A and B to be inelastic in relation to each other, the concession must be strictly confined to the particles themselves; for the gases which are composed of them are elastic bodies: they therefore receive and communicate motion according to the laws which are peculiar to bodies of this description. The foregoing properties of a fluid mixture, which has been supposed to be duly adjusted, is now to be used in the examination of the fundamental proposition of the new theory intended to explain the constitution of the atmosphere. According to this proposition, if two gases come into contact, the particles of which are perfectly inelastic in respect of each other, the particles of A meeting with no repulsion from those of B, further than that repulsion, which, as obstacles in the way they may exert, would instantly recede from each other as far as possible in their circumstances, and consequently arrange themselves just as in a void space. The precedingare the words of the author of the Theory; and it is readily granted that the particles of such a heterogeneous mixture would recede from each other as far as circumstances will permit; the present subject of inquiry then brings the dispute to this issue—can that arrangement take place amongst the particles of two or more gases, which will make their centres of gravity coincide in one point? For the separate equilibria of the fluids, which enter into the constitution of the compound, will not be established until this arrangement be perfectly formed. The completion of this process being essential to the new theory, the effect of it has been, perhaps, too

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hastily inferred in the fourth proposition of Mr. Dalton's first essay; for I am sorry to observe, that the inference is not supported by demonstration, drawn from the doctrine of mechanics. It is the business of the present essay to supply what has been omitted, and to investigate the consequences which must arise from the collision of two heterogeneous gases, differing in their specific gravities.

The existence of the fluid mixture, required by the theory, has been granted already, for the sake of argument; and in order to continue the inquiry, it must be remarked at present, that the necessary internal arrangement of the compound C, is liable to be disturbed perpetually by accidents resulting from the course of things; to which course the author of the theory undoubtedly wishes to accommodate his ideas. The preceding assertion may be exemplified in a manner which is familiar, and may be applied with ease to natural phenomena: let us suppose then an additional quantity of the gas A to be thrown into the pneumatic apparatus, containing the compound C, which was in a state of proper adjustment previous to this event. No one will imagine, that this fresh matter can diffuse itself through the mass of C with the same expedition that the electric fluid shews in expanding along a conductor: this supposition is contradicted by various appearances, from which the following one is selected; agitation is known to accelerate the union of oxigen and nitrous gas. The quantity of A then, which has been newly admitted, will remain at first unmixed with B; but it will act immediately with a repulsive force upon kindred particles diffused through the compound C. This new modification of A will not preserve the density of its parts every where in a constant ratio, to the density of the corresponding parts of B; and this change will disjoin the centres of gravity of A and B; which has been proved above. But when these points are placed apart, the separate equilibria of the fluids cease to exist, which has also been demonstrated before; therefore A and B begin to act and react mutually; which circumstance disturbs the necessary adjustment of C, and forces it to assume another character. It has also been proved in a former paragraph, that the two fluids will act upon each other in the manner of elastic bodies, even when the heterogeneous particles are supposed to be mutually inelastic; consequently A and B will begin to obey the

law of their specific gravities, as soon as their centres of gravity are separated by introducing into the space occupied by C, a fresh quantity of A or B: in consequence of this alteration the centre of gravity of the heavier fluid will begin to descend while that of the lighter moves upwards. When once the centres of two gases are placed apart, their separation will become permanent; because, when at a distance, they are urged in opposite directions by a force resulting from the difference of the specific weights of the two fluids; and this contrariety of efforts must continue so long as the two centres are disjoined, consequently this opposition of force must be lasting; seeing nothing can put an end to it but an union, which it will always prevent. Nor can the mutual repulsion of the constituent particles of each gas, considered apart, in any manner promote the junction of the centres of gravity of the two fluids; because the action and reaction of a number of bodies amongst themselves do not alter the state of their common centre of gravity, whether it be at rest or in motion: so that A and B are under the necessity of observing the law of their specific gravities, just as if the kindred particles of each fluid were actuated by no reciprocal repulsion nor any other cause of reaction. The doctrine of gases, which are mutually inelastic, is rendered indefensible by the preceding arguments; for the hypothesis is thereby exposed to a difficulty which the author of the theory justly remarks, makes a mixture of mutually repulsive gases of different specific gravities an improbable conjecture; so that his own objection ultimately discountenances the leading opinions of that theory which it induced him to adopt in particular. At the same time, philosophers are convinced that the atmosphere is a compound of gases, possessing various degrees of specific weight: they moreover know, that different chemical agents perpetually disturb the equilibrium of the compound, as some of them constantly absorb while others unfold the gases of which it is composed. The preceding facts are certain: consequently the heterogeneous elements of the atmosphere must be united by a common tie, which may be denominated a species of affinity, at least while our knowledge of the subject remains in its present imperfect state. The transparency of the great body of air surrounding the earth, also affords a strong argument for the chemical union of its component fluids; and, at the same

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time, discountenances the idea of the compound being a mechanical mixture of any description whatever; for when a number of diaphanous bodies of different specific gravities are mixed together, they form an aggregate which is opaque; but the union of the substances by fusion renders the mass transparent in many instances. Now as the atmosphere is diaphanous, we are obliged, by the principles of sound argument, to consider it in the light of a compound, the ingredients of which are united by a chemical tie.—Whatever may be the condition of the elastic fluids which enter into the composition of common air, one thing is certain from a preceding paragraph of this Essay; namely, no one of them can maintain a separate equilibrium as long as it makes an individual of the aggregate; consequently, each particle of the compound must be urged by a force resulting from the general action of the mass, not by a pressure occasioned by a particular member of it.

On this account, it is impossible for the acqueous part of common air to preserve the character of a gas at low temperatures; because steam cannot support 30 inches of mercury unless it is heated to 212 degrees of Farenheit's thermometer; were it then practicable to mix vapour of a less heat with atmospherical air, the spring of the gases would reduce it in an instant to the state of a liquid; so that the difficulty, which renders De Luc's theory objectionable in its original form, is not removed in reality by the present modification of it.

The theory of mixed gases has been found to be indefensible on the principles of the mechanical philosophy; and I suspect that part of it which relates to the separate existence of vapour in the atmosphere, will prove equally unfortunate when brought to the test of experiment. Mr. Dalton, in all probability, supposed he had done all that the confirmation of this theory required, by inventing the doctrine of separate equilibria, for nothing more has been offered in support of his opinions, particularly of that relating to the existence of uncombined vapour pervading the atmosphere, unless the statement of the following experiment, with his explanation of it, may be referred to this head. If two parcels of dry air, which are equal in bulk, density and temperature, be confined by equal columns of mercury, in two tubes of equal bores, one of which is wet and the other dry; the air, which is thus ex-

posed to water, will expand more than that which is kept dry, provided the corresponding augmentations of their temperatures be equal; which phenomenon is thus explained on the principles of the theory. The vapour that arises from the sides of the wet tube, possesses a spring of its own; therefore it takes off part of the weight of the mercury from the air, and thereby leaves it to expand itself, so as to re-adjust the equilibrium. According to this explanation, if l and g represent the length of the columns of dry and moist air at any temperature; and if c denote the length of a column of mercury, equal in weight to the pressure that confines the contents of the tubes; and if f be put for the spring of vapour of the same temperature measured by a column of mercury, we have

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$g = \frac{lc}{c-f}$; from which we also get $c = \frac{fg}{g-l}$

the last expression affords us an opportunity of comparing the preceding explanation, and therefore the theory itself with facts; for, according to the experiments of Mr. Schmidt, 1000 parts of dry air at 32 degrees of Farenheit, will expand to 1087,11 parts, by being raised to 59 degrees, in contact with water; call this number g: according to the same author, 1000 parts of dry air at 32 degrees will expand to 1053,61 parts, by being heated to 59 degrees in a dry tube; let this number be l; then $g-l = 33.50$: but f, or the spring of vapour at 59 degrees, is .507, according to Mr. Dalton; then $fg = 551, 164$; hence $c = 16,15$ inches; which expresses the height of the barometer, together with the column of mercury contained in the tube. If the temperature be stated at 95 degrees, c will amount to little more than 8 inches: now it is highly improbable that Mr. Schmidt made his experiments when the barometer stood at a height indicated by either of these numbers.—This application of the theory to practice, affords a presumptive evidence that the principles of it are not altogether just, supposing the experiments of Mr. Dalton and Mr. Schmidt to be correct: but a positive proof of a want of accuracy in these principles may be obtained by introducing a small change into the manner of conducting the experiment made with moist air. This alteration consists in discarding the stopple of mercury, and substituting the simple pressure of the

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atmosphere in the room of it: because when this substance, which is impenetrable to steam, has been removed, the redundant vapour will, according to the theory, flow into the atmosphere, thereby leaving the moist air of the tube to follow the law of expansion observed by dry air. With a view to find whether this be the case or not, I filled a bottle with running water of the temperature of 59 degrees, which, when carefully poured out again, weighed 7794 grains. The bottle, having a dew left sticking to the sides of it, was placed in water at the temperature of 126 degrees: the mouth, which remained about an inch above the surface, was covered with my hand, care being taken to remove it frequently for an instant to permit the vapour and expanding air to escape. After keeping it in this situation about two minutes, I secured the mouth in the manner described above, and inverted it in a quantity of the same water, where it was reduced to 59 degrees; in consequence of which it took up 1622 grains of water, leaving a space equivalent to 6172 grains. If the experiment be now inverted, 6172 parts of air will occupy the space of 7794 such parts when its temperature is raised from 59 to 126 degrees; which is nearly double the expansion of dry air in like circumstances. For, according to Mr. Schmidt's experiments, 1000 parts of dry air of 59 degrees will become equal to 1133,03 such parts, by being heated to 126 degrees; therefore, by the rule of proportion, if 1000 parts give an expansion of 1133,03 such parts, 6172 parts give only 820: but the difference of 7794 and 6172 is 1622, which is nearly the double of 820. The preceding experiment, and others which I have made of the same kind, demonstrate that moist air expands more than dry air under like circumstances; and the fact subverts the notion of uncombined elastic vapour mixing with the atmosphere. The accuracy of the fact may be disputed; the doubt however is removed by repeating the experiment: but so long as my statement remains uncontradicted, the consequences of it to the theory in question, cannot be controverted by argument: for if elastic vapour mix with the air, it does more than merely enter the pores of this fluid; for, according to my experiment, it enlarges these pores at low temperatures, which we know to be impossible, unless the heat of the compound arises to 212 degrees. Those who are

convinced of the superior expansion of moist air, will readily apply the principle to certain interesting phenomena, in particular to the origin of Tornadoes in hot countries, and the variation of the barometer in temperate climates.

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Mr. Barrow, an intelligent traveller in South Africa, observes, that the atmosphere in Caffraria is sometimes heated to 102 or 104 degrees: this is succeeded by local thunder storm, attended with heavy falls of rain and hail, as well as violent hurricanes. I do not pretend to assign the refrigerating cause, or the agent that produces precipitation in this case; I only have to observe, that the portion of air must lose much of its elasticity, which is suddenly cooled to 70 or 72 degrees, and at the same time parts with the water it held in solution. This partial diminution of spring will destroy the equilibrium of the adjacent parts of the atmosphere, and may be supposed to produce the tornadoes of the tropical regions. The same cause probably gives rise to the fluctuations of the barometer in milder climates; for though the changes of temperature are less in the milder than in the hottest parts of the globe, the agents that precipitate the water of the atmosphere, appear to act on a more extensive scale, and through a longer duration in the former situations than they do in the latter. Wet weather is neither momentary nor local in Europe; provinces, and even kingdoms are deluged with rain for weeks together. The air, which discharges such an abundance of water, will lose part of its spring, according to Mr. Schmidt's experiments, even when it suffers no change of temperature: now it is evident that the equilibrium cannot be restored in an instant; because the diminished elasticity must be augmented in this case by currents of air coming from remote places. The diminution of spring in the atmosphere is shewn by the fall of the barometer; and the subsequent ascent of the mercury indicates the arrival of the restorative currents. According to this explanation, the barometer will rise slowly but gradually in the centre of the rainy district, while the motions of it will be more rapid and less regular towards the verge of the storm. High winds will also prevail in wet seasons, which will blow towards the parts where the elastic force of the air is least; that is, where the rains are most abundant.—I know not what claim to originality is due to the foregoing hints towards the

theory of the barometer; they have, however, the merit of being a natural consequence of an established fact; I mean the great dilatation of air saturated with moisture, which must undergo a proportionate contraction when deprived of water.

On the comparative Culture of Turnips. By Mr. WILLIAM WATSON*.

Culture of Turneps.

HAVING been long, and pretty extensively employed in Agriculture, in a district where the turnip husbandry is much practised, and being satisfied that when the soil is proper, and the management judicious, great crops of that invaluable root are the most profitable means of obtaining luxuriant and productive crops of corn, &c. and of laying a solid foundation for future abundance in the increasing quantity of manure, I have paid particular attention to the different modes pursued in its cultivation. It is with great pleasure, therefore, that in the list of premiums offered by the Society, instituted at London, for the Encouragement of Art, &c.—a Society whose patriotic and laudable exertions deserve the most warm and grateful thanks of every real friend to the British empire,—I observe one for the best set of experiments made with a view of ascertaining the most advantageous of these modes; and, having made a comparative trial with great accuracy, I beg leave to request that you will do me the honour of laying this paper, which contains an account of it, before the Society. That there are situations in this kingdom in which eight acres of land may be found of an *uniform* quality, I do not doubt. I must, however, remark, that I never found that number of acres contiguous to each other, or properly situated, for an accurate comparative experiment, in the fallow land of any farm in which I have been concerned, *so precisely similar in soil and condition*, as to induce me to think that I could have exhibited the result of so extensive an experiment as irrefragable evidence of the superiority of any particular mode of culture. Besides I could not have attended either to the minute mixing of the necessary quantity of dung for eight acres of ground, *so as to have rendered it of an uniform quality*,

* Society of Arts, Vol. XXII.

not to the weighing of *all* the turnips upon that quantity of land, without which, (when I adverted to the difference of weight occasioned even by a scarcely perceptible difference in the diameters of similiar solids) I could not have totally divested myself of some doubts as to the accuracy of the result. Cultivtaion of turneps, by Mr. William Watson.

Fo these reasons, I could not *satisfactorily* conduct the experiment on so large a scale as that proposed by the Society; and though I am thereby prevented from becoming a candidate for the Medal,—a reward by which I should have considered myself highly honoured,—yet I hope this Communication will not be deemed altogether unimportant; and that it will, in some degree, forward the views of so distinguished a body.

Every part of the ground upon which this experiment was made, had been managed for a series of years, in exactly the same manner. After being three years in grass, it produced a crop of oats in 1802; in the autum of which year it was once ploughed. In May and June following, it received three furrows in the common way, aud was completely pulverized and cleaned; after which it was divided into four flat ridges, about eight yards broad, each ridge containing precisely 4719 square feet. The soil is a dry, light sandy loam, mixed with small hard stones, incumbent on a thick sudstratum of gravel; and the four ridges were so much alike soil and condition, that I think I may assert, that the most accurate chemical operator could not have proved the smallest difference in these respects. On the 22d of June last, the ridge, No. 1, was manured with dung; immediately after which, the manure was regularly spread over it, and ploughed in. The whole ridge then received a single working, with a light short-tired harrow; and *while the moisture was fresh*, the turnip-seed was sown with a machine, in rows, upon a flat surface with thirteen inches intervals. About the same hour, the ridge, No. 2, was prepared and formed into small ridges, or drills, upon which the turnip-seed was deposited in rows, with a machine, twenty-six inches from each other. *The dung in about one third of the raised drills on this ridge was partly left without being completely covered in.*

Early the next morning, the ridge, No. 3, was also formed into small ridges, or drills, with intervals of twenty-six inches. On the tops of these ridges, a proper machine quickly deposited the turnip-seed in single rows, precisely in the same mode as that pursued in No. 2. On this ridge, however, No.

Cultivation of Turneps, by Mr. William Watson.

3, *every atom* of the dung was carefully covered with the plough. Immediately after No. 3 was finished, No. 4 was dunged and sown with turnip-seed, in the usual manner, in the broad-cast method.—Every part of the four ridges was manured with dung of the same quality. It was not *thoroughly* rotten, but had arrived at a more advanced stage of putrifaction than that used by farmers in general; and, in order that its quality might be uniform, it was carefully taken from *one* part of the fold-yard, and well turned over, and mixed in the field*. An equal quantity was applied to each ridge, at the rate of fifteen two horse cart-loads † per acre. The turnip-seed was likewise of the same quality and kind, and was sown on each ridge at the rate of about one pound and a half per acre. The succeeding weather was remarkably dry and unfavourable for the growth of the turnips, only one light shower having fallen, from the time the seed was committed to the ground, to the 16th of September following.—Notwithstanding this, however, the whole of the four ridges planted exceedingly well, though not so early as I could have wished; and their progress into the rough leaf, as well as their appearance for some time afterwards, was propitious. From the extreme severity of the drought, however, and the natural dryness of the land many of the plants in every ridge were killed. No. 1 lost the greatest quantity; No. 2 the next, *especially on those drills where the dung was not all completely covered in;* and No. 4 scarcely so many as No. 3.—Throughout the whole crop, vegetation seemed extremely languid, and the turnips were generally of a small size; the largest were produced on Nos. 2 and 3, *in the drills with intervals of 26 inches.* These intervals were twice horse-hoe. In these rows the plants were left about eleven inches asunder. Numbers 1 and 4, in which the plants were set out at about twelve inches from each other, were thrice hand-hoed with great accuracy. The several operations of plouhing, sowing, and hoeing, were performed in the same kind of weather on each ridge. I attended the whole of them myself, and can safely say that the utmost precision and impartia-

* Dung was the only manure applied.

† The cart was five feet three inches long, three feet three inches broad, and one foot six inches high, in the inside.

lity were observed.* The four ridges were carefully surrounded with proper rails to prevent damage, and no depredations of any kind were committed *. On the first of this month, *all* the turneps which were produced on these ridges were drawn up, and carefully and exactly weighed, after their tops and tap, or fibrous roots, had been cut off. The produce of each ridge was as under:— Cultivation of turneps, by Mr. William Watson.

	stones.	*lbs.*	*lbs.*
No. 1, drilled on a flat surface, with intervals of 13 inches	144	10	—14 to the stone.
No. 2, drilled on small ridges, with intervals of 26 inches, and with a part of the dung not perfectly covered in	193	5	—ditto.
No. 3, drilled on small ridges, with intervals of 26 inches, and all the dung well covered in	211	4	—ditto.
No. 4, broad cast . . .	162	12	—ditto.

Remarks on the different Modes of Culture.

No. I.

In this method of management the dung is applied in a manner exactly similar to that practised in the broad-cast husbandry; and experienced agriculturists well know, that *even after it has been thoroughly putrefied,* it cannot be *wholly* covered by the earth in the mode of ploughing, pursued under that system of cultivation. In almost all cases, the harrows are used to produce an even surface after the last ploughing, and immediately before the seed is committed to the ground. By this operation more of the dung is left upon the surface; and when it is considered that much of it is applied in a long or half-rotten state, it will readily be conceived, that a still greater quantity will be left exposed on the surface of the ground; in which situation it can conduce but little, if any thing, to increase its fertility.

* Except that a mole destroyed a few plants on three drills on No. 1.

Cultivation of turneps, by Mr. William Watson.

Under this mode of management, the plants may be left at more regular distances in hoeing than in the broad-cast method; but I am *now* inclined to dispute that that operation can be performed at an expence materially, if at all, less than among those obtained in the latter way. The plants are generally left in the rows at about twelve inches apart, so that an acre will produce about 40,200 turneps, when the crop is a full one.

Nos. II. and III.

Some practical agriculturists, as well as chemical philosophers, have contended, that dung should be *thoroughly* putrefied before it be applied to the soil; and others maintain, that it is more beneficial to apply it in a half-rotten state. Into this dispute, I am not, at present, inclined to enter. Let it suffice to say, that a great majority, probably upwards of three-fourths of the farmers, in almost all the extensive turnip districts in the Kingdom, apply it either in the latter state, or before it has arrived at a much more forward stage of putrefaction; and if rotten dung (thoroughly putrefied) cannot be *wholly* covered in this common mode of ploughing, it is obvious, as I have before remarked, that, in the other state, a still greater part must be rendered nearly useless by exposure to the solar rays, &c. In the management now under consideration, however, every atom of it may be buried, if the spreaders and ploughmen are attentive. That management is as follows: As soon as the land has been properly pulverized and cleaned, a double-mould board plough, drawn by two horses, is used to raise small ridges, about 12 or 14 inches high, with intervals of twenty-six inches, and the tops, of about an inch or two broad. All the drills should be equal in size. The height should in some measure be regulated by the quantity and state of the dung. Immediately after the small ridges or drills are formed, a man with a cart, drawn by one or two horses, lays a sufficient quantity of dung for three or five drills (in small heaps), in the interval, while the wheels of the cart run in the adjoining spaces. In this manner all the other intervals are manured. As soon as the dung is carefully spread in the bottoms of the intervals, another double-mould board plough (also drawn by two horses moving in the intervals), splits the ridges along their tops. This operation

completely covers the *whole* of the dung, and reverses the tops and intervals. A roller about ten inches diameter, and four feet in length, drawn by one horse, is now moved along the ridges. It covers two at a time, leaving the tops generally about ten or twelve inches broad, in the middle of which the turnep-seed is deposited, in a rut made by the coulter of the sowing machine, which is fastened to the hinder part of the above roller by a cord about nine feet long; the distance between each row of turnip-seed, being twenty-six inches; and if the ploughing and spreading have been properly performed, the dung will be nearly beneath the rows. Thus the agriculturist is not subject to the waste of any part of his manure, and reaps the superior benefit of having the turnep-seed regularly sown, in a rut of a proper depth, penetrating nearly to the dung in the middle of the small ridges;—a method which seems better calculated to give to the cultivator of the field advantages similar to the rapid and vigorous vegetation promoted by the *hot-bed* of the garden, than perhaps any other mode of culture. The importance of having *all* the dung perfectly covered, is evinced by the result of the above experiment; for, with the exception of a small part of it, in a few drills on No. II., not being perfectly covered with the soil, there was no difference *whatever* between the management of that ridge and the mode pursued on No. III. In dry weather, the roller is moved twice along each ridge, first to compress the soil, and next to close the rut made by the coulter of the sowing machine, to secure the turnep-seed from depredation and drought: but if the soil be so moist as to stick to the roller, it is moved only once along each drill; and some able husbandmen are of opinion, that this is the most advantageous mode *in any state of the soil;* that without the second rolling, the turnip-seed will vegetate regularly; and that, while young and tender, the plants will be beneficially sheltered by the rut of the sowing-machine in adverse weather. Some cultivators form the drills, or small ridges, with a common single plough, and in many situations they are made more straight and neat than with the double plough. With the latter, however, they may, in most situations, be sufficiently well formed, at about half of the expence incurred by using the single plough, *which does not cover the dung better than the other.*—The skuffler, an implement with three or five

Cultivation of turneps, by Mr. William Watson.

Cultivation of turneps, by Mr. William Watson.

hoes is sometimes used to clean the intervals. Some, however, prefer using two small ploughs of the common form, four or five inches broad at the bottom, and fastened together by screws, which increase or diminish their distance from each other, according to the breadth of the intervals. This implement is drawn by one horse, and, by being moved *once* along each interval, cuts a proper quantity of earth from each side of the row of plants; and by proceeding in this manner, a ridge of earth is laid up in the middle of each interval. This mode is the best in situations where the drills are not perfectly straight. Where they are *quite* straight, an implement is used, which, instead of moving the earth from each side of one drill, cuts it off the inner sides of two drills; and in either method the hoeing of the intervals may be performed with equal expedition. A few weeks after these small ridges are formed in the middle of the intervals, they are generally split by a double plough drawn by one horse, the earth being laid close against the turneps on each side. These operations not only destroy the weeds in the intervals, but give to that part of the land the advantages of a bare fallowing, and, besides being *greatly* cheaper, are much more fertilizing than hand-hoeing. In this mode of cultivation the turnips attain a greater size than under the broad-cast method, or that with narrow intervals; and though the plants are generally left at about eleven inches apart in the rows, which reduces the number on an acre, when the crop is a full one, to about 21,900, the result of the above experiment will not be surprising, when it is considered, that from the properties of similar solids, the weights of well-formed (spherical) turneps are in the ratio of the cubes of their diameters, and consequently that one of eight inches and a half diameter will weigh nearly as mnch as three of six inches diameter each.—Nearly all the farmers in this district use their utmost endeavours to obtain turnips of a larger size, which, together with the other important advantages derived from it, has long induced them to prefer drilling on small ridges, with broad intervals, to any other mode of culture; and within the last twenty years, it has become the almost universal practice in the counties of Northumberland, Roxburgh, Berwick, and East Lothian,—an extensive and extremely well managed district; in which, I believe, the rents of land are considerably higher than in any

other in this kingdom. In several, the drills are not drawn at right angles to the ridges (I mean the common ridges of the field), but in a diagonal direction; it having been found, that the seed-furrow in the succeeding spring, together with the effects of common harrowing, not only reduces the land to an *even* surface, but that after such management, the crops of corn are *uniformly* luxuriant and productive, the manured parts being, in these operations, well mixed with the soil in the intervals. I am satisfied, from my own practice, and pretty accurate observation on that of others, that with *considerably less* manure, as weighty a crop of turneps may be obtained by this method of cultivation, as by that with narrow intervals, or in the broad-cast husbandry; and, as it is generally difficult to raise as much dung as will manure the whole of the fallow land, at the rate of fourteen to sixteen loads an acre, this, in *promoting the growth of more extensively luxuriant crops, and increasing the quantity of manure for those which succeed*, is an invaluable advantage. Besides, in unpropitious seasons, when, under the broad-cast and narrow drill system, a judicious agriculturist would not cultivate turnips on land he has not been enabled *thoroughly* to pulverize and clean, he would venture to raise them where the spaces between the rows are sufficiently broad for the admission of the horse and the plough, under an idea that before their tops covered the intervals, (which they generally do about the beginning of October) his ground could be brought into a proper state.—You will no doubt remark, that the crop I obtained even on No. III., was but scanty; and conceive, however, notwithstanding that circumstance, that the experiment satisfactorily shews the superiority of the mode of management pursued on that ridge.—By the same mode, I obtained a crop on the land surrounding that on which the experiment was made, which, considering the extreme dryness of the summer, and that it was sown at the same late period of the season as that upon the experiment ground, may be reckoned a very productive one; and, as the soil was not superior in quality, it may be of some consequence to endeavour to account for this difference. The land marked out for the experiment, contained some couch and other weeds, which I wished to eradicate; it therefore received a common ploughing *only a few days previous to the seed being committed to the ground*. The

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Cultivation of Turneps, by Mr. William Watson.

surrounding land had lain for a much longer time between the last ploughing and the seed-furrow, and contained more moisture at the time of sowing them than the other; and though this, in a humid season, would not have caused a material difference in the crops; yet, in a summer so extremely dry as the last, it was attended with important advantages. To these I may add others; for dung having last year been unusually plentiful, it was manured with about *twenty* loads an acre, and with dung in a very *moist* state; whereas, that applied to the land on which the experiment was made, lost a considerable portion of its moisture by evaporation, during the time of mixing *well*, for the purpose of rendering *all* parts of it equal in quality.—Perhaps it may not be deemed unimportant to state, that the prevailing opinion is, that *very dry seasons are more unfavorable to the turnips raised on the small ridges (drills) than to those produced on land with a flat surface.*

No. IV.

The same objections which have been urged against the manner of applying on No. I. may be advanced against the mode of cultivation pursued on this ridge, under which the plants cannot be left with such precision and regularity as in the drill husbandry.

Expence of each mode of Culture.

The management pursued on Nos. I. and IV., is less expensive *up to the time the plants become fit for hoeing*, than that pursued on Nos. II. and III. This saving of expence, however, is overbalanced by the cheapness of hoeing under the latter mode, and by the advantages derived from that operation being performed before the plants become too large. The general expence of hoeing broad-cast turneps, in this quarter, is about seven to ten shillings per acre, of 4840 square yards. Those in drills, with narrow intervals, will cost as much; and when it is considered, that an acre of these contains twice as many rows as the same quantity of ground under the broad intervals, and that these intervals are quickly and efficaciously hoed with the horse and plough, it will be readily conceived that the latter mode is the least expensive *upon the whole*. As the turneps under this experiment did not grow uniformly,

some parts were much sooner fit for hoeing than others. The person that hoed them was sometimes not employed among them above an hour in the day; which prevents my furnishing an accurate account of the expense of hoeing each ridge.

Cultivation of turneps, by Mr. William Watson.

So easy is the operation of hand-hoeing the small ridges or drills with broad intervals, that in this quarter, it is nearly all performed by women, boys, and girls. If we depended on men, as the farmers do in some other districts, we could not *perfectly* hoe much more than one third of our turnip crops.

I am, Sir,
Your most obedient Servant,

W. WATSON.

North Middleton, near Wooler,
by Belford, Northumberland,
Feb. 18th, 1804.

On Comparative Micrometer Measures. In a Letter from the Rev. Dr. J. A. HAMILTON, *Dean of Cloyne, to the Rev.* J. BRINKLEY, *F. R. S.* *

Observatory, Armach, Jan. 10, 1806.

DEAR SIR,

I BEG leave, through you, to communicate to our Academy the following paper, on comparative observations made with different kinds of micrometers; which, I hope, may be deemed worthy their notice. It was suggested to me, so long since as in the year 1794, that a comparative view of the result of the measures, made under similar circumstances, of the diameters of the heavenly bodies, with the different kinds of micrometers, that are now most generally used by astronomers, might have considerable use; as well in confirming the determinations of the values of the diameters, as given by former observations, as in deciding on the merits of the different instruments, and

Three methods of measuring small angles—the wire micrometer; divided objectglass; and the sextant.

* Irish Transactions, vol. X.

shewing, at one view, a sort of harmony of micrometers. My own opinion, on this subject, entirely coinciding with that of the learned friend, who made this proposal, I set about making comparative observations of the measures of the sun's diameters, as taken with the old wire micrometer, made in the best manner, by Mr. Dolland; with his divided object glass micrometer; and with a ten-inch reflecting sextant, executed, in a very capital style indeed, by Messrs. Troughton.

Description and account of the wire micrometer.

Before I proceed to the detail of the observations, it may be proper to premise a short account of the nature and adjustments of the several instruments, that were the subjects of this experiment. The wire micrometer, as its name denotes, measures intervals, by the separation of two moveable wires: these wires should perfectly coincide, when the index of the scale marks 0 or zero: and the quantity of the separation of the wires, made by the turning of the screw which effects it, is denoted by revolutions, and parts of revolutions, of the index, over a graduated circle, attached to the micrometer-screw; which, in this instrument, consists of fifty sub-divisions. There are several ways of ascertaining the values of these revolutions and sub-divisions, in arcs of a great circle in the heavens. The method which I adopted was this: the microscope being fitted to an achromatic telescope, on an equatorial stand, I carefully separated the wires by fifteen exact revolutions; and then turning round the whole system, till a fixed wire, at right angles to the measuring wires, was in a plane parallel to the equator, I measured, by the sydereal clock, the time the sun's limb, and various fixed stars took, to run along the fixed wire, from centre to centre of the measuring wires. This trial was very frequently and repeatedly made; and the stars and sun's limb, being all reduced to the equator, the general result gave 121",1, for the equatorial interval of the fifteen revolutions. This interval, reduced to space, made each revolution of the figured head = to 2' and 1" of measure; and, of course, each of the fifty sub-divisions = to 2",42 nearly of an arch of the equator. In making the subsequent measures of the sun's diameter, or that of any other celestial arc, the measure was always finished, by moving the wire in the direction in which the fifteen revolutions were originally made. The advantage of this micrometer is

Its scale determined.

principally this: that, in adjusting the telescope, and the micrometer wires, to distinct vision, no alteration is made, by the difference of the conformation of the eye, or of focal distance, that suits that of the observer, in the value of the arc to be measured. The principal defects of it are: the difficulty of judging accurately of bisections, or contacts of the fine wires, by the limbs to be measured; and the impossibility of observing any diameter, except the one perpendicular to the equator.

Its scale determined.

The object-glass micrometer is an instrument, now so familiar to every person conversant in the use of astronomical instruments, that it is only necessary to say, that mine was made, and adapted to a triple object-glass achromatic telescope, of 42 inches focal distance, by Mr. Dolland, and its scale very carefully verified by himself; and that the scale is, as usual, divided into inches, 10ths, 20ths, and vernier divisions: that, when it is applied, it lengthens the focal ditance of the telescope about 6 inches: thus making it 48 inches, or 4 feet focal distance.

Dolland's object glass micrometer.

The advantages of this species of micrometer are: the large scale, the fine images formed, and the facility of measuring diameters in every possible direction. Its imperfections are: that, to different eyes, and under different circumstances of the same eye, the length of the focal distance, that suits distinct vision, will vary; and, of course, the quantity of the measures, given by the scale, are liable to a small variation. The goodness of the telescope is, also, in some degree, impaired, by the application of this contrivance of a divided object-glass.

Its advantages.

It should be noted, that the wire and object-glass micrometers, were both adapted, in their turns, to the same achromatic telescope; and the comparative observations made as near to each other, in point of time, as possible.

The diameters of the sun, measured by the ten-inch sextant, were taken with a small achromatic telescope, magnifying about twelve times, and were observed on the limb, and on the arch of excess, several times alternately; the measures being always finished in the same direction of the micrometer-screw: and the quarter of the double measure was used as the semidiameter, with the addition of 3″; which is the known diminution of the image of the sun's semidiameter, after the reflec-

Ten-inch sextant of Troughton.

tions and refractions it undergoes in the process. As the three kinds of micrometers, just described, are so completely different from each other, in their construction, adjustment, and mode of mensuration, I consider them as fully sufficient to make an experiment on the probable consistency of the results which may be obtained from different good micrometers; and shall now proceed to give a detail of the actual observations.

Observations of the sun's diameter.

1794.	S. Dr. ⊙.	Semidiameters of ⊙, as given in the Nautical Almanack.
August 26, D. O. G. Micr.	15'. 53",835	15'. 53",52.
Wire Micr.	15'. 54",03	

Sept. 3, D. O. G. Micr.	15'. 56",01	15'. 55",4.
Wire Micr.	15'. 55",9	
A set of ten, all agreeing on the sextant, on the limb, and arch of excess	15', 55",0	

Sept. 11, I. T. S. V.		
D. O. G. Micr. 3. 5. 1. 19. Error of V. + 2	15'. 56",54	15'.57",4.
R. D. ⊙. S. Dr, W. Micr. 15. 41.	15'. 57",1	
Sextant, 15'. 55". + 3"	15'. 58",0	

Sept. 17, R. D's.		
Dr. of ⊙. 15. 43. W. Micr.	15'. 58",3	15' 58",9.
Sextant,	15'. 59",3	

Sept. 27,		
D. O. G. Micr.	16'. 1",85	16'. 1",7.
Wire Micr.	16'. 1",95	
Sextant + 3",	16'. 1",70	

Oct, 4, 1794.	S. Dr. ⊙.	Semidiameters of ⊙, as given in the Nautical Almanack,	Observations of the sun's diameter.
D. O. G. Micr.	16′. 3″,76	16′. 3″,6.	
Wire Micr.	16′. 4″,1		
Oct. 6, Sextant ⊙ 3,	16′. 3″,0		

The wire micrometer measures, taken from this time till the next vernal equinox, are omitted; inasmuch, as being taken nearly in a vertical circle, the excess of the effect of refraction on the sun's L. L., required a correction from the tables of refraction; which is liable to some degree of uncertainty at low altitudes. They were found, however, to agree very nearly.

Dec. 14. By a set of measures of the sun's diameters, on the limb and arch of excess, taken, with great care, parallel to the horizon, images extremely distinct, and no discernible spring whatever in the index.

Dec. 15,	
⊙'s S. Dr. + 3″ =	16″.18″,0
D. O. G. Micr.	16′. 17″,83

Dec. 29,		Semidiameters of ⊙, as given in the Nautical Almanack.
D. O. G. Micr.	16′ 18″,9	1′. 19″,2.
Sextant + 3″,	16′ 18″,0	

Feb. 16, 1795.		
D. O. G. Micr.	16′. 13″,45	16′. 13″,7.
Set of good observations with sextant,	16′. 13″,0	

Observations of the sun's diameter.

1795.

March 30th and 31st. Day very favourable; various sets of measures taken with divided object-glass and wire micrometers. The extremes of the divided object-glass micrometer measures never exceeding 1″. Those of the wire micrometer 0.

	S. Dr. ⊙.
D. O. G. Micr.	16′. 2″,45
* Wire Micr.	16′. 1″,9

June 8th. The two different micrometers were applied to the 42-inch achromatic telescope, and the scales verified.

Same day,		Semidiameters of ⊙, as given in the Nautical Almanack.
D. O. G. Micr.	15′. 46″,95	15′. 48″,1.
Wire Micr.	15′. 46″,45	

June 9,		
D. O. G. Micr.	15′. 46″,95	15′.47″,4.
Troughton's sextant + 3″,	15′. 46″,0	

June 15,		
D. O. G. Micr.	15′. 45″,9	15′. 47″,4.
Wire Micr.	15′. 46″,7	

June 19. The measures, with the different micrometers, were taken with the greatest care; and a mean of internal and external contacts, of the sun's limb to the micrometer wires, was used as the measure of the sun's disk by the wire micrometer.

* This curtation of the sun's semidiameter is the effect of the difference of refraction of the L. L. of the sun from the upper.

1795.

Observations of the sun's diameter.

	S. Dr. ☉.	Mean apogeal semidiameter of the sun.	Semidiameters of ☉, as given in Nau. Alm.
D. O. G. Micr.	15′. 45″,26	15′. 46″,38	15′. 47″,0.
* Wire Micr.	15′. 47″,505		

The sextant, on June 25th, shewed, from a careful set of measures, the apogeal semidiameter of the sun, 15′. 44″.

Determination of the sun's apogeal diameter.

On attending to the difference of the sun's apogeal semidiameters, as shewn by the divided object-glass micrometer, and the wire micrometer, I had recourse to some former astronomical records on this subject. By referring to De la Lande's Astronomy, article 1387, I find, that, in the year 1759, De la Caille observed the apogeal semidiameter to be 15′. 47″.2; and that De la Lande, in 1760, made it 15′. 45″25.

These two measures happen to correspond so exactly with mine, as made with the different micrometers, that it may be a matter of some consequence, to inquire, what kind of micrometers they used to deduce their respective semidiameters.

It is unnecessary to extend these observations any farther. I shall, therefore, only add to this paper, that it will appear, by comparing the divided object-glass micrometer's measures of the sun's diameters, of Decembers 15, 1794, and of June 19, 1795, that the difference of the perigeal and apogeal diameters of the sun was found to be 65″,14. De la Lande found this difference 64″,8. but he calls it, in round numbers, 65″.

Note. Where no notice is taken of the time of observations, it is to be understood they were taken very near to noon, and as soon after each other, as micrometers could be changed.

The originals of these observations, and several others, are

* This measure comes nearer to the calculated apogeal semidiameter of the sun than the former; but as, at the making of these observations, the state of the air caused the sun's limb to undulate, perhaps the divided object-glass micrometer, having a much greater magnifying power, than was used with the wire micrometer, its observations may have been rendered more uncertain.

to be seen in the registry of observations kept at the Observatory, Armagh, for the years 1794 and 1795.

I have the honour to be,
Dear Sir,
Your faithful and obedient Servant,
JAMES A. HAMILTON.

Observations on the Metallic Composition for the Specula of reflecting Telescopes, and the manner of casting them: also, a Method of communicating to them any particular Conoidal Figure: with an Attempt to explain on scientific Principles, the grounds of each Process: and occasional Remarks on the Construction of Telescopes. By the Rev. James Little *

Excellence of the Reflecting Telescope.

THERE are but few things produced by the united effort of mechanical artifice and intellectual labour, which have done more honour to the ingenuity and invention of man, than the reflecting telescope; which has many advantages over any of the dioptrical kind, notwithstanding their improvement by acromatic glasses. It will bear a greater aperture, and may be made to magnify more, (as being more distinct,) in proportion to its length, than the others, as they are at present made; and its dimensions and powers are unlimited. What its excel-

Particularly the Newtonian.

lence is, especially the Newtonian construction of it, has been proved by Dr. Herschell, to his own honour, and that of the age, and country, and patronage, which encouraged his labours. Accordingly, the persons, eminent for science and mechanical ingenuity, appear to have felt a peculiar and disinterested pleasure, in contributing to its improvement: and the late discovery of a metallic composition for the mirrors of it, which will bear as high a polish as glass, reflect as much light as glass transmits, and endure almost equally well, without contracting tarnish, is a farther encouragement to prosecute its improvement to perfection.

* Irish Transactions, Vol. X.

Among others, I had formerly, from admiration at its contrivance, bestowed some attention on the mechanism of this instrument: and, as it would have spared me some expence of time and trials, if any other person had previously suggested to me the hints, which I am to relate; I imagine they will be of use to others, in directing or assiting the course of their labour, in the same pursuit. I had also taken some pains, to understand the merits of the different constructions of this telescope: but, as this inquiry ended in a conviction, that the Newtonian form of it is the most perfect that can be hoped for; (it being the nature of its great author to persevere in his researches, till he had arrived at a complete solution of his doubts, and comprehension of the subject;) so I have only to report what resulted from my experience in the mechanical fabrication of it, as to the method of casting the mirrors, and communicating to them the proper figure.

Introduction respecting its Structure.

Before I had heard of the improvements of the Rev. Mr. Edwards, in the composition of the specula for telescopes, I bad made many experiments myself with that view; which lead me to give full credit to his report of the superior excellence of that composition which he recommends: because I had found, that the qualities of hardness, whiteness, and indisposition to contract tarnish, necessary to a speculum, could not, by any admixture that I could hit upon, be produced, unless the metal were so highly saturated with tin, as to be excessively brittle; and because I found that this brittleness, however inconvenient in some respects, was necessary to render it susceptible of the highest polish: for no metal yet known, except steel, (which, from its disposition to rust, is unfit for this purpose,) will take as high a polish as glass will, unless it be more brittle than glass. And indeed this property is common to all substances which we know, that are capable of such polish: they must be very hard, and, as such, brittle; for the polishing powder employed would stick and bed itself in any soft metal, instead of cutting and polishing it.

On the nature of the Metal.

From the result of my trials, I contented myself with the composition mentioned hereafter, as being in every respect sufficient for the purpose, and inferior to none in whiteness, lustre, and exemption from tarnish: for, as to the addition of silver, I found that, when used even in a very small quantity, it had an extraordinary property of rendering the metal so soft, that I was

Silver renders it soft; but the trials were not extended.

deterred from employing it: and unless it shall be found that, without this effect, it makes the metal less porous than otherwise it might be, or less frail and brittle, I am certain that it may, in every other respect, be dispensed with. I had no opportunity to try it, in the precise quantity Mr. Edwards recommends, (though I did so before, in very nearly that proportion,) since I first saw his memoir on that subject. Sir Isaac Newton made trial of a very small portion of it, and found the same effects from it as I experienced: but it is possible, that, if it were added in the just proportion discovered by Mr. Edwards, it would be an improvement, and useful ingredient, in the composition *.

The metal liable to tarnish from imperfect casting.

I must observe here, that a metal, not liable to contract tarnish from the air, is otherwise susceptible of it accidentally; when there happen to be minute holes in its surface, caused by the air, or sand, &c. in casting it. Such cavities will be filled with the dust, or rusty solution of the brass, in grinding; which will, in time, become a sort of vitriol, and act on the contiguous parts of the speculum, producing a canker in it, which will spread, in form of a cloud of tarnish, around each cavity. In such a case, to prevent this, I would advise, to lay the mirror, as soon as polished, in warm water, and, after drying, while it remains heated, to rub it over with spirit-varnish, from which it may be cleansed, by a piece of fine linen dipped in spirit of wine. The varnish will remain in the cavities; and, by defending the impurities in them from the action of the air, will probably preserve them from becoming corrosive to the metal.

The Composition, copper, brass, tin, silver, and arsenic.

From numerous experiments, of the qualities of different compositions, made by several persons, it appears, that no combinations, of any other metals or semi-metals, are fit for specula,

* Having read somewhere, that zinc and gold made the best speculum-metal, I tried it; and found, that the zinc was sublimed from the gold in fusion, and arose to she top in the crucible, forming a white, hard, spongy mass. The metal, called tutanag, is fit for specula, when melted with tin; but I am certain, that what I procued, under the name of tutanag, was a mixture of brass and copper, &c.; for the zinc, in the brass, rose from it, during the fusion, in white flowers.

except those of copper, brass, tin, silver, and arsenic. I tried no semi-metal, except the latter, which whitens copper, and unites intimately with it: because it is stated, in the treatise of the *Art of Assaying*, by the observant and accurate *Cramer*, that all the semi-metals rise in flowers, during the fusion: which would certainly make the metal porus. On this account, I would have rejected the brass, because of the zinc contained in it; but that it seemed to render the composition whiter, and less apt to tarnish, than it would be without it. It will have little tendency to rise in flowers, if the speculum-metal be fused, with the lowest heat requisite, and if the brass be of the best kind; because, in this, the zinc is more perfectly united with the copper, and both are purer. I used, for this purpose, the brass of pin-wire: and, because the quantity of it was only the one eighth part of the copper employed, which, I imagined, would receive too fierce a heat, if put alone into the melted copper; I first added to the brass, in fusion, about an equal quantity of the tin, and put the mass cold into the melted copper; supplying afterwards the remainder of the tin, and then the arsenic; the whole being generally in the following proportion: viz. 32 parts best bar copper, previously fluxed with the black flux, of two parts tartar, and one of nitre, 4 parts brass, 16½ parts tin, and 1¼ arsenic. I suppose, with others, that, if the metal be granulated, by pouring it, when first melted, into water, and then fused a second time, it will be less porous than at first.

The brass.

Proportions adopted.

In this process, whatever metals are used, and in what proportions soever, the chief object is, to hit on the exact point of saturation of the copper, &c. by the tin. For, if the latter be added in too great quantity, the metal will be dull-coloured and soft; if too little, it will not attain the most perfect whiteness, and will certainly tarnish. It is too late to discover the imperfections of the metal, after the mirrors are cast and polished; and no tokens given of them (that I know) are sufficiently free from ambiguity. But I observed the following, which proved, in my trials, at first view, indubitable marks of the degree of saturation; and I think it fit to describe them particularly, as they have not, to my knowledge, been noticed by others.

How to determine the Saturation of the tin and copper.

When the metal was melted, and before I poured it into the flask, I always took about the quantity of an ounce of it, with a

small ladle, out of the crucible, and poured it on a cold flag; and observed the following appearances:

From a cast Specimen.

First. If the metal assumed, in cooling, a lively blue, or purple colour, commonly intermixed with clouds, or shades of green or yellow; and if, when broken, the face of the fracture exhibited a silvery whiteness, as bright and glistening as quicksilver, without any appearance of grain, or inequality of texture; then the degree of saturation of the metal, with the tin, was complete and perfect.

And the fracture and colour.

Secondly. If the surface of the metal became of a dun or mouse colour, and especially if of a brown or red; and, when broken, the fracture exhibited a more yellow, or tawny hue, than that of quicksilver; then the quantity of tin in the composition was deficient, and it was necessary to add more.*

Thirdly. If the colour was an uniform dull blue, like lead, where broken, discovered a dull colour, with a coarse grain, like facetts; the due saturation was exceeded, and there was an over proportion of tin in the metal.

Explanation of the colours.

These colours would be more distinct, if a small quantity of the metal were cast in a flask, which had been previously smoaked, by a candle, made of resin mixed with tallow; in which way I used to prepare the moulds. I attribute the formation of the colours to this: that, as the calx of every metal has its own peculiar colour, so, the heat of the melted mass, calcining some of the particles on its surface, which are in contact with the air, these display the colour of the calces of those ingredients which prevail in the composition. Whence, it may be expected, that, if the copper is the redundant metal, the mass will exhibit a reddish tinge, which is appropriate to the calx of copper; and, if the tin be prevalent, a blueish die ought to appear. Either of these colours, therefore, appearing unmixed, shews the redundance of that metal, to which each belongs. And, as brass, when cast alone, has always a

* This can always be done by degrees, and without any trouble, till the point of saturation is found; whereas, if too much tin were added at first, there would be a necessity for melting more copper separately, and repeating the whole process: and different specimens of copper will require different proportions of tin; so that the due quantity can never be known, *a priori*, but on trial only.

yellow tinge, so, when these three colours are exhibited in a cloud-like mixture, they shew an equality and due proportion of their respective metals in the composition. When too large a mass of the metal is cast together, its intense and lasting heat calcines the surface so deeply, as (when exposed to the air) to obscure the colours; so that a small quantity will best serve to exhibit them.

Method of casting.

As to the method of casting the mirrors, it has been directed, to leave the ingate, or superfluous part of the cast, so large, as to contain a quantity of metal, equal to that in the mirror itself; which would occasion a great waste of it, and render it not easy to cast, at once, more than one mirror in each mould; and even this might be done so injudiciously, as not to afford security against a miscarriage of the cast. But it will appear, that this great quantity of metal and incommodious manner of casting it, are by no means necessary. However, a judgment cannot be formed, of what may be the safest and most eligible method for casting the mirrors, unless it be considered, what are the circumstances attending this operation, in the case of malleable metals; and how the management of speculum-metal, in this respect, must differ from that of them: since there must be peculiar difficulty in casting, in sand, a metal more brittle than glass.

Large ingate?

Effects of the contraction of fused metals,

When any fused metal is poured into the flask, the external parts of it, which are in contact with the mould, congeal and harden sooner than the internal parts, and form a solid shell, filled with the rest of the metal, in a fluid state. This will, consequently, remain in a state of greater expansion, from its heat, than the external crust; and its particles will, in the act of shrinking as it cools, recede from one another, as being more easily separable, and cohere, on each side, with the particles already fixed and grown solid: by which means a vacuum will be formed in the middle, and this will be gradually filled by the superincumbent metal, which has been later poured in, and remains longer in a fluid state. But, when there is no more metal supplied, the void, which was in this way latest formed, remains unfilled; and then the shell of the metal, adjacent to the vacuum, as yet remaining soft, and unable to bear the weight of the atmosphere, resting on it, sinks, and is pressed down into the vacuum: by which means,

a pit or cavity will be constantly and necessarily formed in the face of the cast, in that part of it which was last congealed; which cavity will commonly be larger or smaller, in proportion to the quantity of metal in the cast.

—particularly in speculum-metal.

The event will, in this respect, be the same with speculum-metal, as it is, in the case of that which is tough and malleable: only that, as the former, in cooling, arrives sooner at its natural state of hardness and brittleness, its external solid shell will not bend, but break, and fall into the void part under it; and thus form cracks, or abrupt chasms, in the places, where tougher metals would contract only regular depressions. And also, when the body of the cast is small, or the mould is so damp or cold, as to congeal, not only the surface, but the substance, of the cast too soon, and thus prevent a gradual influx of the fluid metal, to keep the central part as distended, as the exterior shell was, when it became fixed; the farther contraction of the interior parts of this brittle, refractory metal, after it has become solid, will be apt to form rents in it, because its substance will not bear extension, without rupture.

Remedy: by a supply of the melted metal,

It would be an obvious remedy of the above inconvenience, if there could be contrived a reservoir of fluid metal, to descend into the interior part of the cast, and fill up the void made in it, as fast, and as long, as it is forming by the contraction of the metal. Now, this is effected, by having a jet or appendage to the cast, of such a size, form, and position, as will be effectual to retain the metal, composing it, in a state of fluidity; and also to suffer it to descend into the interior of the cast, until all parts of the same become fixed, and incapable of receiving any farther influx of metal. For thus, all the imperfections, that would otherwise be in the cast itself, will now exist only in the appendage to it, which is a supernumerary part, to be afterwards seperated from it. This appendage ought to be of the form of a prism, and as nearly that of a cube, as the operation of moulding it in the sand will permit; for, in this gross shape, the metal in it will be the longer cooling. It should be connected with that part of the mirror, which is uppermost in the flask, and joined to it by a neck, equal in thickness to the edge of the mirror, (but so posited, that the face of the mirror may project a little above it), and, in breadth, about twice the thickness. This neck ought to be as short as possible, i. e. just so as to

—in the form of a prismatic appendage.

permit it to be nicked round with the edge of a file, in order to break off the prism from the mirror when cast: for thus the heat of the large contiguous body of the prism will keep the neck from congealing; which, if it happened, would stop the liquefied metal, in the prism, from running down into the mirror. And, to prevent this, the prism ought not to form directly a part of the main jet or ingate, by which the metal is poured into the flask; for so the jet would cool sooner than the large mass of the mirror, and bear off the weight of the atmosphere, which ought to press on the fluid metal in the prism underneath, and force it down into the mirror, to fill up all vacuities in it. Both the prism and the mirror, therefore, ought to be filled by a lateral channel, opening (from the principal ingate) into the top of the prism; which latter should be formed broad and flat, and not taper upward, like a pyramid, lest, by cooling where it grows narrow, it might form a solid arch, and oppose the pressure of the atmosphere. When it is fashioned, as here directed, and made of a bulk equal to a third or fourth part of the mass of the mirror, or even a fifth or sixth part, when the mirrors are of large size, there will ever be found, in the top of the prism, after the metal is cast, a deep pit or cavity, which contained the metal, that had ran down into the mirror, after the outer shell of the mirror, and sides of the prism, had become solid and congealed; and the mirror itself will be found perfect, without any sinking or cavity; which could only be formed by an injudicious disposition of the jet or appendage, permitting the metal in it to freeze sooner than the whole mass in the mirror, and thus stopping its descent into it. If several mirrors be cast together, in the same flask, there must be such a separate appendage made to each of them.

In this manner I have (without a failure in any) cast many mirrors of different sizes, and sometimes several of them together in one flask. But very small ones, such as the little mirrors for Gregorian telescopes, cannot be cast in this manner; for their masses being but small, they cool too quickly, to receive any additional infusion of metal; and their outer edges, suddenly forming a solid incompressible arch, the central parts, in contracting towards it on every side, separate, and are rent asunder. And this has happened, even when I cast them in brass moulds made red hot: on which

The small speculums to be made out of a bar.

account, I have been obliged to form them out of pieces of the metal, cast in long thin ingots or bars; which, by nicking them across with a file, could be easily broken into square pieces, whose corners could be taken off, and rounded in the same manner.

Reference to Edward's Treatise. See our Journal, quarto series, vol. V.

I do not repeat the other precautions to be observed in this process, which have been already so well and sagaciously described by the Rev. Mr. Edwards: but the circumstances above mentioned, a prudent attention to which, is, in my opinion, essentially necessary to the success of it, are not to be collected from any directions published on the subject that are known to me. And though particular artists may, by large experience, arrive at a sufficient knowledge in this matter, for their own practice; yet, to render that knowledge general, and to contribute, as far as I could, to the improvement of this instrument in any hands, being the design of this essay, I thought it necessary to state the above particulars fully; though I doubt not that these, as well as other matters of moment in the operation, are known to many, who chuse not to make them public. Thus the great skill, in the construction of the telescope, acquired by Mr. Short, seems not to have been transmitted to any successor.

Figuration of the mirrors.

I come now to speak of the most difficult part of the mechanism of this instrument, that of communicating a proper figure to the mirrors; on which depends the powers of the telescope, when its dimensions are given: for the manner of polishing them, to the highest degree of lustre, has been already well understood and described. They who have tried this part of the work, and know how inconceivably small is that incorrectness of form, which will produce grievous aberrations of the rays of light, will, I am sure, readily subscribe to the assertion, that '*hoc opus, hic labor est.*' Methods have indeed been proposed for accomplishing it; but not a single hint given, that I know, of the *modus operandi*, or the grounds of these methods: insomuch, that, when I first tried to polish mirrors, I had no idea why any figure of them, different from that of a sphere, should result from the modes of polishing recommended. But, on my making the attempt, in the ways proposed by Mr. Mudge and by Mr. Edwards, I was surprised to find, that sometimes a spheroidal or other irregular figure, and sometimes (though rarely) a conoidal one, was produced

by each: the cause of either being to me then unknown; and disappointment or success appearing to depend on mere accident, and not on the degree of pains and accuracy used in the process.

At length I began to suspect, that these variations, in the event of the process, (which will be hereafter accounted for,) arose from some property, not adverted to, in the pitch that covered the polishing tool; which material has been generally used for this purpose, of communicating a proper figure, as well as a high polish, to the mirror, since it was first recommended by Sir Isaac Newton; being commonly spread on the polisher, to about the thickness of a crown-piece, and then covered with the polishing powder: (the manner of doing which I suppose the reader to be acquainted with, as also with what has been made public on the subject, by Messrs. Hadley, Mudge, Edwards, &c.;) and I was confirmed in my suspicion, from the following reasons, after I had found them approved by many repeated and diversified experiments.

The methods of Mudge and Edwards vary from the properties of the pitch.

Pitch is a soft unelastic substance, which, as such, will suffer a permanent change of form, when it is made to sustain a degree of pressure sufficient to communicate an intestine motion to its particles: and this property directs us to consider, what may be the effect of the pressure of the mirror on it, when spread on the polisher, as to the figure it may then gradually acquire, during the operation of polishing, and the resistance and friction it will oppose to the mirror: for, by reason of the tenacity of its substance, it will resist a certain degree of pressure, without change of its form, but will yield to a greater pressure. But it is by its resistance the mirror is worn down and polished; if, therefore, that resistance be not uniform and equal, on the whole surface of the polisher, neither will the abrasion of the mirror be equal in every part; the consequence of which must be, that both will degenerate from an uniform curvature, i. e. from a spherical figure; the mirror from unequal friction, and the polisher from its mobility, by which it will adapt to the successive alterations produced in the figure of the mirror; their mutual action and reaction inducing a change in both.*

The pitch, though hard, is yielding.

* This change, however, being so little, as to be imperceptible by the senses, and, in the imagination, referable to various other

The law of its giving way.

As the pitch is (in our present inquiry) to be considered as an homogeneous substance, we must suppose, that its resisting force, as well as that of the pressure of the mirror on it, are uniformly diffused over the surface of the polisher: and, from hence, it may not, perhaps, be easy to conceive, how the surface of the mirror could sustain from it any inequality of resistance and friction. In fact, these would be equal and uniform, in every part, if the pitch were a substance, either of perfect hardness, or perfect fluidity: but it will hereafter appear, that its consistence must not be so hard, as to render it incapable of any change of form; but, on the contrary, so soft, as to yield, in a small degree, to the pressure of the mirror: at the same time, opposing a resistance, sufficient to wear down and polish it: and the inquiry is, how that resistance is modified.

Explanation. It recedes uniformly,

Bodies of perfect hardness, such as glass, flints, &c. will not admit a total intimate change of their form, in all their dimensions, without a dissolution and permanent separation of all the particles composing their masses, (except when they are brought to a state of fusion by heat). But soft, viscid, semifluid bodies, such as lead, pitch, &c. will suffer such change, preserving the cohesion of their particles, yet, at the same time, undergoing a general intestine motion of all the par-

causes, it becomes necessary, in order to establish the true cause, not only to deduce its existence and effects solely from reasoning on physical principles, but also to obviate other different conjectures that might be formed, by stating fully those circumstances that take place in this operation; and which, indeed, are necessary to be clearly understood in judicious practice. Both these ends cannot be answered, in a disquisition new and intricate, without a minute explanation: and this, I hope, will be received as my apology, for the prolixity of this account, which I would gladly have curtailed, if I knew how to do so, without making it less intelligible or useful to the practical optician. This class of readers will forgive any diffusiveness on a mechanical subject, if the perusal may tend to spare them the greater labour of fruitless experiments; or afford any hint towards conducting them more judiciously: and as for their use this paper was designed, I have adverted to such various matters as I thought most worthy their attention; and which yet have not been so fully and familiarly explained by others, as they ought to be, for the instruction of an artist.

ticles among themselves: so that the coat of pitch, pressed, on each side, between the parallel surfaces of the mirror and polisher, will, by their force, be equally extended laterally in every direction; by which an equal quantity of motion will be communicated to all its particle: since no particles, except those at the extremities, can move, without protruding others, and these, the rest, successively, as if the mass were a fluid body.

—but with different resistance at different parts of the surface.

But, though all parts of the surface of the polisher receive an equal pressure and motion, all do not exert an equal degree of resistance to that pressure: for those parts, that cannot move without displacing and overcoming the resisting tenacity of a greater quantity of the surrounding mass of pitch, than other parts do, must oppose the greater resistance to the mirror, as having that of the other parts superadded to their own. For ascertaining this, the force impressed, and the quantity of pitch, confining any annular tract of the polisher, should be computed. In the present case, where the coat of pitch is a thin equal statum, of circular form, we need regard only its superficial dimension, and consider all parts of it as alike situated in the above respect, which are equidistant from the center, or from the outer edge of the polisher.

Deduction of the quantities of reaction or pressure.

To this purpose, let the surface of the polisher be conceived to be composed of an indefinite number of concentrical zones or annuli. Each of these will sustain an uniform pressure, from the mirror, proportional to its area, because, the force impressed on the mirror, and its attraction to the polisher, is equally diffused on it. The areas of those annuli, taken separately, are the differences of the two circles, whose peripheries inscribe and circumscribe each of them; and they are consequently to each other, as the differences of the squares of their diameters, or as those of their radii; and the series of them taken, in order, from the center to the extremity, are strictly as a rank of figurate numbers proceeding from unity, viz. the odd number 1, 3, 5, 7, &c. But, since their breadth is supposed to be infinitely small, they may be taken as proportional to their mean diameters or radii, i. e. as their distances from the center of the polisher; which distances will, therefore, represent the pressure on each annulus, and the quantity of motion communicated by that pressure; seeing it must be, as

the number of particles the annulus contains that are moved; i. e. as its area.

This resistance may be raised, But the resistance to the force impressed on any annulus, being as the quantity of pitch to be put in motion by it, will be different, not only as the annulus is nearer to, or farther from, the margin of the polisher, but different, also, as this has either one margin only, or two, i. e. when the polisher is entirely covered with pitch, or when it has a space left uncoated at the middle: which latter always is, and must be the case, when the great mirror of the Gregorian telescope is to be polished, which has a perforation at its center.

—by taking away some of the surface of the pitch at proper places. First. When there is no vacant space in the middle: the resistance to the several annuli will be as the circumambient spaces only; because, the pitch not being compressible, it is only into these, and not towards the center, it can, in yielding to the force or weight of the mirror, extend itself, by lateral motion: and the space, surrounding any annulus, is the difference between the circular area of the polisher, and that inscribed in the annulus; and is, relatively to the rest, measured by the difference of the squares of their radii, viz. of the distances of the edge of the polisher, and that of the annulus, from the center. But since, in this case, the bodies (of pitch) are unelastic, there can be no augmentation of motion; nor can the quantity of motion and action communicated, and, consequently, the resistance to it, and reaction, exceed that which is impressed: on which account, I imagine, that the resistance to the several annuli is to be taken as proportional to the pressures they sustain, and measured by them, i. e. by their magnitudes or areas, or the number of particles in them, to which a motion is imparted; which were stated to be as their respective radii or distances from the center: and, consequently, I suppose the resistance to be the inverse of this, or as the distances of the annuli from the outer edge of the polisher; which distances measure the direct resistance, or the quantity of pitch, to which equal motion, with that in the respective annuli, is communicated.

Effect of a central hole, And from hence it follows, that, if a mirror previously ground to a spherical figure, were to be polished on such a polisher as this: the resistance and friction of the pitch, being greatest, and increasing to a maximum at the center, and diminishing towards the extremity, would wear down and

polish the mirror, most in the central part, and least towards its edges; thus giving to it a curvature, the reverse of a conoid, which it ought to have, and which it can never at first acquire correctly, by any other mode of polishing, but that of wearing it most down (and thus reducing its curvature), towards its extremities.*

Secondly. When there is a hole made through the center of the polisher, or a void space left there, uncoated with pitch.†

In these circumstances, the pitch will have liberty to expand itself (when yielding to the pressure of the mirror), towards the center, as well as the edges of the polisher: and, as the resistance and friction, in any annular tract of it, is as the direct extent of pitch, bounding it on either side, it follows, from what has been laid down, that it will encrease in any part, as the distance of the same annulus encreases, from each extremity of the coating of the polisher; and will be in a ratio compounded of the distances, from the interior and exterior margins of the pitch. So that, if the breadth of the polisher between these margins were, (for example,) 5 inches: then the pressure and friction in the middle tract, equidistant from the outer and inner edges, would be, to that prevailing at the distance of half an inch from either margin, as $6\frac{1}{4}$ to $2\frac{1}{4}$, (nearly as three to 1;) and the same, at proportionate distances, in polishers of any other size; which unequal pressure could never produce, in the mirror, a regular curvature of any species; and, in the spaces nearer to the margins, the inequality of pressure would be still greater. Whence may be conceived the impossibility of figuring mirrors correctly, on polishers disposed in this manner, without some remedial contrivance; whether the face, or area of them, be of a circular shape, as directed by Mr. Mudge and others, or oval, as proposed by Mr. Edwards: for the mirror would be thus least reduced, and left of a spherical form, at the middle and edges; and be worn down, and hollowed into a different and irregular curvature, in the intermediate tract.

—shewn in the figure produced on such a polisher.

* It will be hereafter shewn, for what particular purpose, solely, such a polisher may be employed.

† There ought always to be a hole made through the polisher to prevent the confinement of air or water, near the centre of it.

Remedies.

For these inconveniences, however, arising from the unequal friction of the polisher, there are the following easy and adequate remedies; which will, in the sequel, be more fully explained, and applied as in practice, to effect the degree of curvature, or any correction of the same, which may be requisite.

Make the polisher larger than the mirror.

First. Since the curvature of the mirror ought to be gradually reduced towards its edges, which can only be effected by an increase of friction in the corresponding part of the polisher; and that this latter effect is to be produced in any part of it, by enlarging the surrounding coat of pitch: it follows, that, for this purpose, the breadth of the polisher must be enlarged above that of the mirror; and this in the same degree, as the curvature of the mirror is to be diminished: so that the polisher is to be of greatest breadth, for a mirror of an hyperbolic, and least, for one of a spherical figure. This, however, is to be done, under the limitations hereafter mentioned.

Contract the center hole.

Secondly. To preserve the regular gradation of curvature towards the middle of the mirror, the uncoated space, at the center of the polisher, should be contracted to a certain limit, which will be defined; though, for the reasons above mentioned, it can never be filled up altogether.

Cut out some of the face of the polisher where the reaction is greatest.

Thirdly. Where the resistance and friction of the pitch, in any tract on the face of the polisher, is computed as above, or found in effect, to be too great; it may be lessened and regulated, in any degree, by cutting, out of that part of its surface, some of the pitch, at proper intervals, in narrow channels or furrows: the number and depth of which ought to be proportioned to their distance from the edges of the coat of pitch directly, and to the reduction of curvature, proper to the corresponding parts of the mirror inversely, and should be in a ratio compounded of both; for, by these cavities, the continuity of the pitch being dissolved, its resistance, depending thereon, may be modified at pleasure.

In this way, the small, as well as the large speculums, may be duly figured.

In this manner may the polisher be so disposed, as to communicate a correct figure to large mirrors, and even to those of smallest size. Now, whatever success may have attended the efforts of other persons, in communicating a proper figure to the *great* speculum, (especially Mr. Short, whom I have manifold reasons for believing to have been among the most

eminent opticians, as well as artists, that have laboured in the improvement of this instrument;) I have not heard, that any method has been proposed, of communicating, to the *little* mirror of the Gregorian telescope, any other than a spherical form, which yet may in this manner be done. And it must, in this telescope, be a thing most desirable to accomplish; especially when its size and aperture is so great, that it would be difficult to impress, on the extensive surface of its great mirror, (merely by the small alteration of figure, which could be produced, in the delicate operation of polishing,) the degree of change, from its prior state of spherical curvature, which would be requisite; since the defect of form, in this mirror, may, in these cases, (as will be shewn,) be easily compensated, in the figuration of the little mirror. For the greater size of this latter, in such instances, will render it capable of more steady handling and motion, and more equal pressure; and so more manageable, and susceptible of a correct figure, in proportion as the encreased magnitude of the great mirror renders it unmanageable: which is, plainly, a great advantage, in the fabrication of this telescope; whose mirrors will thus, in the cases where it is most especially necessary and desirable, admit mutual correction and compensation for each other's defects.

The principles, or physical causes, operative in this process, as above stated, seem to be incontrovertibly evident; and, as I am not aware of any paralogism admitted in the reasoning upon them, I must suppose, that a mode of operation, conformable to these principles, is the thing chiefly requisite to ensure success. In this view, I have attempted to conduct the process; and, as the almost insuperable difficulties attending it are felt, even by those whose inventive powers and resources ought to afford the highest hopes of accomplishing the object, and yet disappoint them in their attempts at high perfection;* so I, among others, may be allowed to state the

Difficulties of the processs.

* Sir Isaac Newton, who had himself laboured in this undertaking, of polishing the concave mirror of his own telescope, and with such talents for the work, and such success, as to discover that method of doing it, which has, to this day, been followed, observes, (to use his own words) that "optict instruments "might be brought to any degree of perfection imaginable, pro-" vided a reflecting substance could be found, which would po-

difficulties, that, to my apprehension, occurred in the enterprize, and to obviate objections; as, from hence, there may be suggested some hints, to facilitate or abridge future labour to others, or to prevent hopeless trials.

Arguments and inferences respecting the process of polishing mirrors.

I must observe, then, that different effects must necessarily follow, from using, in the process of polishing, pitch of a softer or harder consistence. If the pitch be of a temper quite hard and unyielding, no part of the surface of the mirror can be made to suffer a higher degree of friction than the other parts of it, unless these latter parts be elevated and detached from the face of the polisher, and disengaged from contact with it; because, in this case, both mirror and polisher are supposed to preserve their general shape regular and unaltered; and therefore, the contact, and, consequently, the friction, must be either complete and equal, on the whole surface, or none at all. For, if we suppose, that, by the wearing down

" lish as finely as glass, and reflect as much light as glass transmits, and the art of communicating to it a parabolic figure be also attained. But there seemed (*said he*) very great difficulties, and *I had almost thought them insuperable*, when I farther considered, that every irregularity, in a reflecting superficies, makes the rays stray five or six times more out of their true course, than the like irregularities in a refracting one; so that a much greater curiosity would be here requisite, than in figuring glasses for refraction. . . . . &c.

" But having afterwards thought on a *tender way of polishing*, proper for metal, whereby, as I imagined, *the figure would also be corrected to the last*, (i.e. to the utmost) I began to try what might be effected in this kind; and, by degrees, perfected an instrument . . . . &c. . . . . and afterwards an other one."

The tender way of polishing, which Sir Isaac Newton here mentions, was (as he afterwards described in his Optics,) to cover the polisher with pitch: and he declares, that he imagined the *figure*, as well as the *polish*, would by means of this, be perfected. I cannot help thinking, that this extraordinary man, who was born to anticipate others in invention, as well as discovery, had the same ideas as are here detailed, though he did not explain, nor, perhaps, succeed in, the application of them in practice: for he states, (in his Principia) that a spherical mirror will reflect the oblique pencils, issuing from the extremities of the field of view, as truly as a parabolic one, and seems to despair of effecting a more correct figure.

of the mirror towards the extremities, it is made gradually to change its spherical form, the part of its area, so abraded and diminished, cannot subside into a state of actual contact with the polisher, unless the other parts of it are elevated and disengaged from the polisher, at the same time; or unless it may be imagined, that the particles, worn off the mirror by friction, are applied and adhere to the corresponding parts of the polisher, so as to raise and augment its surface, just as much as that of the mirror becomes depressed and reduced. If this effect could be supposed to take place, it would follow, that, in every variety in the direction of motion in the mirror, the friction must tend to wear down the edges, rather than the middle of the mirror; because the motive force is always applied to a part of the handle to which the metal is fastened, raised more or less above the surfaces in contact. The effect of which must be, to communicate to the foremost or advancing half of the mirror's surface, a pressure downward, on the face of the polisher, equal to the force expended in moving the mirror forward; and thus to abrade and reduce the several parts of the mirror's surface, proportionally to their respective distances from the center; by which its curvature will be made to approach to that of a parabola, by its wearing down most towards the edges: and this, weather the motion be conducted in lines diametrically across the polisher, or with round strokes; so as that its center should describe, every time, a little circle, about the center of the polisher. This is, however, entirely on the supposition, that the edges of the polisher become raised, by the adhesion of the dust worn from those of the mirror: for, if this were not the case, but that the polisher were to retain its spherical form, while that of the mirror was altered, the contact could not be general between two surfaces of dissimilar shape. If these adhered together in one part, they must be dissevered in another: and the force, necessary to seperate them in this latter part, which can never be greater than that required to move the mirror forward, must yet be more than equal to the force of cohesion, in the part of the mirror, which, in each stroke, is to be disengaged from the polisher. This pressure is found, in the case of bodies in contact, to be incomparably greater than the weight of the atmosphere, which is equal to about seventeen or eighteen pounds on every square inch of the surface of the mirror:

Arguments and inferences respecting the process of polishing mirrors.

Arguments and inferences respecting the process of polishing mirrors.

and, when this latter is brought so near that of the polisher, as to suffer friction from the powder bedded in it, their mutual attraction will amount to a much greater force than is requisite to move forward the mirror: no part of which can, therefore, be disengaged from the polisher, nor, consequently, be unequally worn down, so as to produce, in its surface, a form different from a spherical one, or from that of the polisher.

This reasoning and conclusion will equally stand, whether it be supposed, that the force of cohesion is confined to the very surfaces in contact, or extends to a little distance from them, diminishing in the duplicate, or any other ratio of that distance; and that the bodies are not wholly removed out of the sphere of attraction when there is a small interval between them. For, as this force is greatest at the very surface; so, the bodies in contact cannot be disjoined at all, to the smallest distance, but by a force superior to the whole cohesive force.

It may, perhaps, be imagined, that the pressure of the atmosphere ought to be taken into consideration, and be added to the force of cohesion, which keeps the surfaces in contact with each other. But this pressure acts as much upon the coat or plate of water, which must be interposed between the surfaces of the mirror and polisher, as upon these surfaces themselves: and, because the pressure upon any part of a confined fluid, is propagated to the whole of it, in every direction; so, the weight of the atmosphere, resting on the edges of this fluid plate, tends as much, by the interposition of the same, to buoy up, and force assunder, the surfaces resting on it, as it does to compress together these surfaces, by its action on themselves; and exerts itself equally to prevent their approach on one side, as their recession on the other. I conceive the agency of these forces to be this: that the plate of water is so strongly attracted by she surfaces nearly in contact, as to be kept from running off, and has its outer edge exposed to the weight of the air; whose pressure is thus communicated to all the particles of the water, and, by its mediation, to the contiguous surfaces of the mirror and polisher. And, though all these are really compressed together, by the surrounding atmosphere, yet I conceive that this does not hinder their gradual separation from being effected: because, as fast as that separation takes place on any side, the air and water rush in between the surfaces, to fill up the vacuity, as

it is formed; and no farther resistance arises to their disjunction, than what is owing to the viscidity of the fluid interposed, and to the force of cohesion; which latter acts, in this case, quite different from any external force of compression; and prevails, as I apprehend, to a small distance from the surface, diminishing in the ratio of some high power of that distance. *

Arguments and inferences respecting the process of polishing mirrors.

And hence I suppose, that the weight of the atmosphere is wholly inefficient, in keeping the mirror and polisher in mutual coherence, when any liquor of perfect fluidity is between them; and that the force of cohesion acts alone to this effect. Accordingly, it is found, that, when the polisher is so much wetted with water, that there is formed a continuous plate of this fluid between it and the mirror, an additional force, sufficient to squeeze out the water interposed, becomes requisite to bring the surfaces into actual contact, and to produce so much friction between them, as will serve to wear down and polish the metal; which process will be found, in these circumstances, to advance very slowly and irregularly. And, on the contrary, when so little water is applied to the polisher, that it is only made damp, and scarce wetted, (i. e. when there is not a coutinuous body of liquid interposed between it and the mirror,) then its contact with the metal will be so intimate and strong, that the latter will polish very quickly. For then their surfaces approach within the sphere of the attraction of cohesion: insomuch that, if all moisture were suffered to evaporate, the mirror and polisher would cohere so firmly, as not to permit any friction, or even a separation of their surfaces, and the polisher would be destroyed; for then the weight of the atmosphere, also, would be superadded, when no fluid is interposed:

* If it were supposed, that the force of cohesion is confined to the surface of bodies, and acts only in the state of actual contact; it would be hard to conceive, why a drop of liquor should ascend, in a conical glass-pipe, whose narrow end was elevated: since the drop ought, on this supposition, to be attracted as much by the surface below, as by that above it: and its weight ought to make it descend; and there would be nothing to make it spread beyond the space of contact which it occupies: whereas if the attraction extends, directly in right lines, to a distance from the sides of the pipe, the composition of their forces ought to make the drop ascend, and spread itself in its course, as it happens in fact.

all which shew that their cohesion, when a fluid does intervene, is not caused by the pressure of the atmosphere.

Agreeably to this, the sagacious Newton directs, that, towards the end of the operation, no more moisture should be applied to the polisher, than what it will contract, from the operator's breathing on it. Indeed, a person, who has formed a just conception of his genius and intense application of mind and considered the hints and precepts he has given in this work, can hardly doubt, that he could, and, perhaps, would, have furnished a theory of the rulesand method of this whole process; had he not imagined it would, at that time, be regarded as a matter of too little importance, to deserve so minute an explanation, which must be necessarily prolix, and seem unworthy of him, who was occupied in more sublime speculations.

Hard pitch will not give a good figure,

From this it follows, that, when the pitch is of unyielding hardness, it will not, in any mode of polishing, communicate to the mirror the desired shape, if the dust worn from the mirror, does not alter the shape of the polisher. And, as this seems not likely to happen, so I was not surprised, that my efforts, to effect the desired figuration of the mirror, by using very hard and refractory pitch, failed of success.

And there is this inconvenience, moreover, in the use of such pitch, viz. that it makes so great resistance to the sinking and bedding of the polishing powder in it that the particles of the powder, however fine it may be, will, on any fresh application of it, or when any grains of it are accidentally dislodged from the pitch, roll about loose on the polisher, and scratch the face of the mirror, so as to destroy the polish before given; thus making any fresh application of the powder inadmissible, unless the pitch were to be softened by heating it, which would destroy its former figure, and render the operation uncertain and tedious. It was to allow the polishing powder to fix itself, without rolling loose on the polisher, and to suffer all its particles, however different in size, to sink in it, so as to form an even surface, that Sir Isaac Newton, in his sagacity, employed a coat of pitch on the polisher, as a soft substance, that would yield to the powder, when impressed on it by the mirror, and not afford such resistance, as to make it fret the face of the metal; and also as a substance endued with another property equally necessary, that of being perfectly unelastic. For no elastic substance will ever communicate an exquisite polish to

—nor will it receive the polishing powder.

a metallic speculum, though it would to glass, crystal, or jewels; because no metal can be cast, perfectly free from small pores: and any elastic substance, if employed to polish it, would insinuate itself, together with the polishing powder, into these pores, and wear down their edges in such a manner, as to convert every pore into a long furrow or cavity: which would occasion the destruction of the whole surface of the metal, as was truly observed by Sir Isaac Newton. And thus it appears, that, to make the pitch too hard and refractory, would be to destroy every property in it, which renders it eligible in this operation.

The polisher must change its figure while that of the mirror changes.

If the positions, before stated, be well founded, it seems to follow, that the desired change in the mirror, from a spherical to a conoidal figure, can only be effected, by a change in the shape of the polisher, gradually accommodating itself to the alteration, produced in that of the mirror, during the process of polishing. Nor, indeed, can it well be conceived, how the mirror could alter its spherical form, if that of the polisher remained unaltered; for a conoid could never, in the usual way, and without a partial separation of the surfaces in contact, be polished on a segment of a sphere, nor even on that of a conoid, if, during the friction of their surfaces, the center, or vertex of the one, were to be moved to any considerable distance from that of the other. So that the strokes, in polishing, must never ultimately be carried so far as to remove the center of the mirror to too great a distance from that of the polisher; even though its surface were so hard, as to preserve its figure unaltered by the pressure of the mirror.*

* For the several reasons above mentioned, I am inclined to think, it will be very difficult to discover a method, different from that here explained, of communicating, at the same time, a perfect figure and polish to a speculum. It is plain, that Newton could think of no better; though I imagine that, in this instance, he tried his inventive powers with those of Des Cartes, who had published a method (in theory elegantly geometrical) of figuring optic glasses. And I cannot dissent from those, who think this was the method employed by Mr. Short, with such success, in figuring the mirrors of his telescopes; I mean a conduct in the operation, sagaciously adapted to the properties of the pitchy coating of the polisher.

It must be obvious to the reader, that none of the remarks or directions, contained in this essay, can be meant to apply direct-

The pitch should be a little harder than common.

Agreeable to these positions, I found, in my trials of polishing mirrors in the common way, by straight or round strokes of the mirror, on the polisher, that the operation was more easy and successful, when I used pitch of nearly the common consistence, than when I employed such as was made very hard, by long boil ng it, or by the addition of much resin. Such softer pitch will admit more than one application of the polishing powder, without scratching the metal, or spoiling its previous polish; by which means, the process will be more expeditious. It will instantly accommodate itself to the successive alterations in the form of the metal; as this, by wearing down towards its edges, gradually changes from a spherical, to a conoidal shape: and it will promote this effect, by opposing a greater resistance to the

ly to the polishing any speculum, whose magnitude is too great, to admit of being moved on a polisher, of equal size with itself. Where the friction, and force of cohesion, of such large surfaces in contact, and the weight of the mirror, exceed the motive power that can be employed, a polisher, of less extent than the whole surface of the mirror, must be applied, to traverse, in succession, the several parts of it; and the motion must be given, not to the mirror, but to the polisher. Instruments of far less enormous magnitude than Doctor Herschell's great telescope, are *sui generis*, and require particular methods of polishing the mirror adapted to their size. For such, no person should presume to propose any method, which he has not approved in practice: though, as the general principles here laid down, are, with due accommodation, applicable to a polisher of any shape or extent of surface; it should seem, that, if such great mirrors could be polished by a regular and uniform motion, their polishers might be made such segments or sectors, &c. of the area of each respective mirror, and of such breadths in different ports; and the furrows, made in the coating of pitch thereon, of such number, proximity, and depth, as to afford, in the tract of the motion of each part, a degree of pressure and friction, reciprocally proportional to the degree of curvature, proper to each concentric zone of the mirror's surface; which would tend to produce the desired figure, so far as a polisher, covered with pitch, could be made instrumental to this purpose. For though the size and shape of the polisher were to remain unaltered, yet its resistence and abrading power might be considerably modified, by varying the number and depth of the furrows, made in the pitch which covers it. And the effect of a process, thus conducted, will be commensurate to the time it is persisted in.

metal, and greater friction towards its extremities, when its previous disposition on the polisher has been judiciously provided, in the manner before explained.

Description of the kind of stroke most proper for giving the figure.

But, to fulfil these intentions effectually, a certain kind of motion, of the mirror on the polisher, must be carefully observed, during the operation: for, as the softer pitch will continually yield, and sink under the pressure of the metal; so, the form of the polisher, degenerating in every stroke, must be recovered, and preserved correct. According to the principles before laid down, the face of the polisher must be considerably larger than that of the metal, in order to afford a greater resistance to the speculum, towards its extremities: so that, as the metal covers only a part of the polisher, if the former were to be confined in its motion, the pitch, sinking under it, would expand itself laterally, and become heaped up suddenly, around the tract of the mirror's pressure; which must, therefore, to obviate this, be so conducted, as to traverse, in quick and regular succession, every part of the polisher, in order to recover the regularity of its figure as fast as it becomes vitiated. And this is effected in two ways: either by enlarged circular strokes of the metal, brought considerably beyond the edges of the polisher, in order to repress, towards the center, the pitch, which had become raised near its edges, or by straight diametrical strokes, across its surface, in every direction successively: either of which will tend to preserve the figure of the polisher, and, consequently, of the mirror, nearly spherical. As, however, a spherical figure is not that which is ultimately intended, so these modes of conducting the process are to be pursued only till the mirror has acquired a sufficient polish, and a figure nearly spherical: and then, in order to give it a parabolic or hyperbolic shape, the motion of the mirror, on the polisher, should be such, as that the center of it may describe a spiral line round the center of the polisher, by enlarging the circular strokes, till the edge of the mirror arrives at the edge of the polisher; and then contracting the motion gradually, till the mirror returns to the center, in the same spiral course. By which means, any sudden and irregular elevation of the pitch, beyond the place of the mirror, will be prevented: while, at the same time, it will become regularly elevated, from the outer edge, in the form of a conoid, and

thus be adapted for communicating the same figure to the mirror.

I have been led to adopt and practise this method of polishing mirrors, by the train of reflections and reasoning herein described, and with sufficient success, for its unreserved recommendation. In one particular, it corresponds with the method published by Mr. Mudge, in the Philosophical Transactions, viz. in the direction of the motion used in polishing the mirror. But this seems to have been prescribed by him, without any respect to the properties of mobility and inequality of friction, in the pitchy coating of the polisher; which things he has not noticed. And yet, as any sort of motion, without a proper regard and adaptation to the qualities of the pitch, would be ineffectual, it is here attempted to supply that defect; because no method can be rightly pursued in practice, nor its success be uniform, nor any figure already given to the mirror be altered, if those artists, who would follow it, are ignorant of the principles and agency on which it is really founded. For, in every process of so subtil and delicate a nature, some untoward accidents and circumstances must occur, which will grow above the control and correction of any person, who is not aware of the secret causes from whence they arise. In such cases, the practice will be as imperfect as the theory is.

Method of taking away part of the face of the polisher.

It has been above explained, how the middle zone, or tract of the polisher, equidistant from its inner and outer edge, when there is a void at the center, will oppose a greater degree of friction to the mirror, than the other parts of the polisher. And, to prevent the unequal wearing of the mirror, by the increased action of this zone, it will be proper, that, agreeable to the methods of prevention of this effect before mentioned, there should be circular furrows indented in the pitch within this zone, more or fewer, according to the size of the mirror, and the designed degree of its curvature; in order that the pitch may subside into the furrows, and thus the resistance and friction in that tract may be diminished. This will be very easily accomplished, by putting the polisher on the arbor of a lathe, and cutting out some of the pitch in circular grooves, with a small sharp and concave turning chisel, wetted with water, in which some soap has been dissolved. And this may be performed and repeated,

if necessary, without any injury to the surface of the polisher, if it be previously wetted, to prevent the splinters of the pitch from sticking to it; which may be washed off, by a soft brush or pencil, from the polisher, it being immersed in water.

It is better that the small speculum should be spherical.

Since, in the Gregorian telescope, the defect of figure or curvature, from that of a conoid, in one of the mirrors, may be compensated by a contrary curvature in the other; and since, in either of the mirrors, whose breadth is given, the degree of variation in its figure, from that of a sphere, ought to be so much the greater, as its focus, or radius of curvature, is shorter; it will, on this account, be far more difficult, to effect a proper figure of the small mirror in this telescope, than of the large one; because the former must be a greater segment of the sphere, than the latter. For which reason, instead of making the one of an elliptic, and the other of a parabolic form, I imagine it would (with the exceptions before mentioned) be more proper to rest content with a spherical form in the little mirror, (by which means, several of these latter, being fastened, with cement, beside each other, on the same handle, might be accurately and easily ground and polished together, on one tool and polisher, made sufficiently large); and to employ the great efforts on the large mirror, in rendering it of an hyperbolic form; which is not at all more difficult than it is to make it parabolic: for, on account of the small extent of surface of the little mirror, it is very difficult to govern and regulate its motion and pressure, so as to communicate to it any certain figure, if polished by itself singly; as it must be, when it is to be of any other than a spherical form. Yet, even this may, by an intelligent and dexterous artist, be accomplished, to a considerable degree of perfection, in the manner above mentionned, as I have repeatedly experienced; though the process is much more easy and certain, in figuring the large mirror (under that limitation of its size before intimated): for the greater the surface to be polished is, the less will any inequality of pressure, in the operation, alter the form of the mirror, or the polisher; such inequality, being a part only of the motive force employed; and the more extensive the surface is, the less proportion does the motive force bear to the force of cohesion, which tends to preserve an uniformity of pressure in the mirror, and of figure

—but the conoidal figure may be given.

in the polisher. And I believe it is on this account, rather than that of preventing aberrations of the rays of light, from a supposed spherical shape of the mirrors, that telescopes of greater apertures and foci are more accurate; the larger surfaces of their mirrors having a tendency, during the operation of polishing, to preserve the regularity of their figure. For, let the aperture of a telescope be ever so large, with respect to the focus of the great mirror; yet when the object is very remote, the central part of the field of view (the rays of light from which are parallel to the axis,) ought to appear perfectly distinct, if the metals were wrought up to the correct figure of conoids: and the vulgar doctrine of aberrations, which relate only to spheres, is entirely inapplicable. The only standard, for the measures of the apertures and foci, is the degree of ingenuity in the workman, who fabricates the instrument. There are many defects in figure, besides a spherical form of the mirrors; and it happens but too frequently, that a telescope is very indistinct, from a bad figure of them, though that figure is the nearest to a conoid of any regular curve: for this is often the case, when the central, the extreme, and the intermediate parts of the mirror, successively and separately exposed to receive the light from the object, appear to have the same focus. And this mostly occurs, when the mirrors are small; certain tracts, or portions of their surface, being more worn down, by the grinding or polishing, than others, arising from the difficulty of preserving an uniform pressure during the operation, and, consequently, a regular figure of the polisher.

A larger speculum is most easily figured.

Another method, different from that now described, of communicating to mirrors a parabolic form, has been discovered by the late Rev. M. Edwards, and published in the Nautical Almanack, for the year 1787. He recommends, to make the edge of the polisher the periphery of an ellipse; so that the face or area of it may not be round, but oval: the shortest diameter of the ellipse being equal to that of the mirror; and its longest diameter to be to the shortest, as 10 to 9. And he affirms, that a mirror, finished on such a polisher, will prove to be of a parabolic form; if the process be conducted, by employing, throughout the operation, straight strokes of the mirror, diametrically acros the polisher, in every direction. Now in the method recommended by M. Mudge, whatever

kind of motion be used, in bringing the face of the mirror to a polish, the parabolic form is directed to be acquired, only by a circular motion in polishing: Mr. Mudge having declared, that the effect of such straight strokes would be, to produce no other than a correct spherical figure in the mirror. Here, then, are opposite motions, and declared to be productive of contrary effects, proposed by two very intelligent artists, with a view of promoting the same effect; the only difference being this, that, in the one case, the face of the polisher is supposed to be round, and in the other, oval: a difference that a person may well imagine to be (as it really is) of very little importance; and he may be easily led to suspect, that the presumed effect of either mode is only imaginary; that a spherical figure of the mirror has been mistaken for a parabolical one: or that, if the latter has been produced, it may have been, not by method, but by chance; and he may naturally distrust any rule or method advanced for this purpose. Thus, when different instructions are given, by different persons, without any reasons or explanations assigned as the foundation of them, the whole rests on authority; authorities clash, and then the worst may be followed, or all be rejected; and, for want of a guide, an uncertain practice be adopted. It is for this reason, I have judged it necessary here, (as also in former essays, made public,) to be very minute, in attempting to investigate the grounds of any method to be pursued, and the principles of action, in the operation of the instruments I am treating of.

Commendation of the method of Edwards.

I have made a trial of the method of polishing, proposed by Mr. Edwards, with attention to all the circumstances, which he directs to be observed; and, from the result, I have reason to believe, that his method is a good one, and will, if judiciously applied, produce as correct a figure of the mirror, as, perhaps, any other, yet made public. But, whoever will attentively investigate the nature of the operation, will, I think, cease to wonder, that modes of conducting it, seemingly so dissimilar, tend to the same effect, and perceive, that the contrariety is not real, but merely apparent *.

For, in either method, it is not the direction of the motion

* In the methods of figuring the mirrors, published by Mr. Mudge, and by Mr. Edwards, it is stated by Mr. Mudge, that he frequently, during the process, applied to the polisher a concave

The radial and the spiral stroke compared.

employed, nor the shape of the area of the polisher, which, in reality, produces a conoidal form in the mirror; but a gradual alteration in the curvature of the face of the polisher, by yielding of the pitch, under the pressure. And, therefore, when any part of the area of the polisher, whether it be round or oval, is more extended than that of the mirror; the pitch, moving laterally, will become elevated, and its curvature lessened, in that part. So that, in a polisher of oval shape, whose conjugate diameter is equal to that of the mirror, the pitch will ascend and accumulate, in the part, which lies without the circular area of the mirror, inscribed in the ellipse. The extremities of the mirror will, therefore, be worn down, when each part of them is made in rotation, by straight strokes across the polisher, in the transverse diameter of the ellipse, to traverse that part of it, which circumscribes the circle; and, by such strokes made twice, directly in that diameter, and oftener obliquely, in each rotation of the mirror, as the operator moves round the polisher, during the process, the regular shape of the polisher is preserved. But it is easy to conceive, that the same effects would follow, though the polishing were conducted, not by straight strokes across, but by round strokes, in a spiral direction, as above mentioned. And I am doubtful, to which of these motions the preference should be given; or whether they ought not to be interchangeably used, to produce the most elaborate form in the mirror; as also, whether this method, of Mr. Edwards, is better than the former, by Mr. Mudge, above described. For I have been deprived of leisure and opportunity (by the war, and public troubles, during the French invasion and the rebellion; in which, most of my instruments, for such purposes, were lost, in the plunder and

Doubt which may be preferable.

tool, which he calls a bruiser; by which he must have preserved or recovered, the figure of the polisher, and, consequently, of the mirror, that otherwise must have become vitiated, by the unequal resistance of the pitch; and Mr. Edwards made furrows in the coating of pitch, or his polishers. It is to these circumstances, and not to the direction of the motion employed, or the elliptic area of the polisher, that, I can think, was owing to the success, attendant on their methods: the bruiser being necessary, to supply the defect of furrows in the pitch; and the oval form not essential, when there were such, duly disposed, and also the polisher of proper size, &c. as here directed.

destruction of my house,) to prosecute the experiments, which might have enabled me to speak with more precision; and which I would have done, from the desire I had, to contribute to the perfection of so noble an instrument as the reflecting telescope.

I know, that both methods will, in judicious practice, produce the desired effect; but this effect will be limited, in degree of perfection, and sometimes frustrated, when the causes and circumstances, that operate in it, are unknown. In either method, and with a polisher of round or oval shape, it is indispensably necessary, that there should be furrows made in the coating of pitch, (to allow it to subside, in regular gradation, from the middle to the edges,) by indenting it, either in squares, as is usually done, or in circular channels; both which must be renewed, as they become filled up and obliterated; which will always happen soonest in the middle zone or tract of the polisher, between the center and other edge, whether the furrows be circular or longitudinal: and, if this be not done, the regularity of curvature would not be preserved in the mirror, or the polisher. But, since there is no obstacle to the subsidence of the pitch, near its outer edge, and its inner edge, when there is a void space at the center, I believe the furrows ought not to be made there, but in the intermediate space only. And I am of opinion that it is, from the judicious disposition of these furrows, the most correct shape of the mirror is to be acquired, whether the polisher be round or oval, or the pitch hard or soft: for I found, that, in Mr. Edwards's method, and with pitch, even as hard as he recommends, the channels made in it were, towards the end of the operation, nearly obliterated, in the middle zone of the polisher. But this will not happen so soon, nor so dangerously, with hard as with soft pitch; nor will the correction of the impaired shape of the polisher be so difficult, when it is of an oval, as when of a circular area: there being, in the former case, less of irregular surface in it, to be reduced; and a more steady, uniform, and simple motion, in grinding, may be pursued; which, as it will admit of a less degree of expertness and sagacity in the artificer, will be more commonly attended with eminent success*. (*To be Continued.*)

Both may succeed.

—but these must be furrows in the pitch.

* I imagine, that a polisher, whose area is of an oval form, would be better adapted to the formation of a parabolic, than an

VI.

On the inverted Action of the alburnous Vessels of Trees. By THOMAS ANDREW KNIGHT, Esq. F.R.S. *From the Philosophical Transactions*, 1806.

Theory deduced from facts, by the author: that the sap circulates through the leaves and descends through the bark.

I HAVE endeavoured to prove, in several Memoirs* which you have done me the honour to lay before the Royal Society, that the fluid by which the various parts (that are annually added to trees, and herbaceous plants whose organization is similar to that of trees), are generated, has previously circulated through their leaves † either in the same, or preceding

hyperbolic curvature, in the speculum; and that the latter will be most correctly formed by a polisher, whose area is nearly circular. For, in order to make the speculum hyperbolic, the longest diameter of the oval polisher must be considerably greater than the shortest one, i. e. than the breadth of the mirror: as will be evident, from a consideration of the circumstances I have endeavoured to explain. And, as the mirror must be carried, by the strokes in polishing, to the extreme verge of the polisher; so, when it is to traverse it, in the direction of its longest diameter, it will have its center or vertex removed too far from that of the polisher, to acquire from it a true conoidal figure. Either, therefore, the face of the polisher should be round; or, if it be oval, it ought to be rendered a less eccentric ellipse, by having its shortest diameter greater than that of the mirror, which will allow the extent of the polisher to be reduced, by contracting proportionably its tranverse diameter; i. e. it must be brought nearer to a circular figure. For the objection, mentioned by Mr. Edwards, to a round shape of the polisher, when it is to be considerably larger than the mirror, viz. " that it makes the latter perpetually into a " segment of a larger sphere, and by no means of good figure," I apprehend to have chiefly arisen, from an omission, in those who tried it, to make furrows in the pitch, in the proper tract, on the surface of the polisher; which, if it had been done, would have produced, not a spherical, but a conoidal figure.

* In the Phil. Trans. for 1801, 1803, 1804, and 1805.

† During the circulation of the sap through the leaves, a transparent fluid is emitted, in the night, from pores situated on their edges; and on evaporating this liquid obtained from very luxuriant plants of the vine, I found a very large residuum

season, and subsequently descended through their bark; and after having repeated every experiment that occurred to me, from which I suspected an unfavourable result, I am not in possession of a single fact which is not perfectly consistent with the theory I have advanced.

Apparent objection from a fact stated by Hales and Du Hamel.

There is, however, one circumstance stated by Hales and Du Hamel, which appears strongly to militate against my hypothesis; and as that circumstance probably induced Hales to deny altogether the existence of circulation in plants, and Du Hamel to speak less decisively in favour of it than he possibly might otherwise have done, I am anxious to reconcile the statements of these great naturalists, (which I acknowledge to be perfectly correct,) with the statements and opinions I have on former occasions commuicated to you.

That the stem below an annular privation of bark, lives and grows, though little.

Both Hales and Du Hamel have proved, that when two circular incisions through the bark, round the stem of a tree, are made at a small distance from each other, and when the bark between these incisions is wholly taken away, that portion of the stem which is below the incisions through the bark continues to live, and in some degree to increase in size, though much more slowly than the parts above the incisions, They have also observed that a small elevated ridge (*bourrelet*) is formed round the lower lip of the wound in the bark, which makes some slight advances to meet the bark and wood projected, in much large quantity, from the opposite, or upper lip of the wound.

Explanation of this fact.

I have endeavoured, in a former Memoir,* to explain the cause why some portion of growth takes place below incisions through the bark, by supposing that a small patt of the true sap, descending from the leaves, escapes downwards through the porous substance of the alburnum. Several facts stated by Hales seems favourable to this supposition; and the existence of a power in the alburnum to carry the sap in different directions, is proved in the growth of inverted cuttings of different species of trees.† But I have derived so

to remain, which was similar in external appearance to carbonate of lime. It must, however, have been evidently a very different substance from the very large portion, which the water held in solution. I do not know that this substance has been analyzed, or noticed by any naturalist.

* Phil. Trans. for 1803. † Ibid. for 1804.

many advantages, both as a gardener and farmer, (particularly in the management of fruit and forest trees,) from the experiments which have been the subject of my former memoirs, that I am confident much public benefit might be derived from an intimate acquaintance with the use and office of the various organs of plants; and thence feel anxious to adduce facts to prove that the conclusions I have drawn are not inconsistent with the facts stated by my great predecessors.

The first motion of the true sap in germination is downwards.

It has been acknowledged, I believe, by every naturalist who has written on the subject, (and the fact is indeed too obvious to be controverted,) that the matter which enters into the composition of the radicles of germinating seeds existed previously in their cotyledons; and as the radicles encrease only in length by parts successively added to their apices, or points most distant from their cotyledons, it follows of necessity that the first motion of the true sap, as this period, is downwards. And as no alburnous tubes exist in the radicles of germinating seeds during the earlier periods of their growth, the sap in its descent must either pass through the bark, or the medulla. But the medulla does not apparently contain any vessels calculated to carry the descending sap; whilst the cortical vessels are, during this period, much distended and full of moisture: and as the medulla certainly does not carry any fluid in stems or branches of more than one year old, it can scarcely be suspected that it, at any period, conveys the whole current of the descending sap.

—through the bark.

Cortical vessels from the bases of the leaves, by which the sap descend.

As the leaves grow, and enter on their office, cortical vessels, in every respect apparently similar to those which descended from the cotyledons, are found to descend from the bases of the leaves; and there appears no reason, with which I am acquainted, to suspect that both do not carry a similar fluid, and that the course of this fluid is, in the first instance, always towards the roots.

The ascending sap passes through the alburnum and central vessels.

The ascending sap, on the contrary, rises wholly through the alburnum and central vessels; for the destruction of a portion of the bark, in a circle round the tree, does not immediately in the slightest degree check the growth of its leaves and branches: but the alburnous vessels appear, from the experiments I have related in a former paper,* and from those

* Phil. Trans. for 1804.

I shall now proceed to relate, to be also capable of an inverted action, when that becomes necessary to preserve the existence of the plant.

As soon as the leaves of the oak were nearly full grown in the last spring, I selected in several instances two poles of the same age, and springing from the same roots in a coppice, which had been felled about six years preceding; and making two circular incisions at the distance of three inches from each other through the bark of one of the poles on each stool, I destroyed the bark between the incisions, and thus cut off the stem and roots, through the bark. Much growth, as usual, took place above the space from which the bark had been taken off, and very little below it. Experiments with young oaks.

Examining the state of the experiment in the succeeding winter, I found it had not succeeded according to my hopes: for a portion of the alburnum, in almost every instance, was lifeless, and almost dry, to a considerable distance below the space from which the bark had been removed. In one instance the whole of it was, however, perfectly alive; and in this I found the specific gravity of the wood above the decorticated space to be 114, and below it 111; and the wood of the unmutilated pole at the same distance from the ground to be 112, each being weighed as soon as it was detached from the root.

Had the true sap in this instance wholly stagnated above the decorticated space, the specific gravity of the wood there ought to have been, according to the result of former experiments,* comparatively much greater: but I do not wish to draw any conclusion from a single experiment; and indeed I see very considerable difficulty in obtaining any very satisfactory, or decisive facts from any experiments on plants, in this case, in which the same roots and stems collect and convey the sap during the spring and summer, and retain, within themselves, that which is, during the autumn and winter, reserved to form new organs of assimilation in the succeeding spring. In the tuberous-rooted plants, the roots and stems which collect and convey the sap in one season, and those in which it is deposited and reserved for the succeeding season, are perfectly distinct organs; and from one Experiment with the potatoe.

* Phil. Trans. for 1805.

of these, the potatoe, I obtained more interesting and decisive results.

My principal object was to prove that a fluid descends from the leaves and stem to form the tuberous roots of this plant; and that this fluid will in part escape down the alburnous substance of the stem when the continuity of the cortical vessels is interrupted: but I had also another object in view.

The early varieties afford neither blossoms nor seeds, because the early tubers consume the true sap which might have formed them.

Every gardener knows that early varieties of the potatoe never afford either blossoms or seeds; and I attributed this peculiarity to privation of nutriment, owing to the tubers being formed preternaturally early, and thence drawing off that portion of the true sap, which in the ordinary course of nature is employed in the formation and nutrition of blossoms and seeds.

Cuttings of the potatoe managed so as to produce no tubers,

I therefore planted, in the last spring, some cuttings of a very early variety of the potatoe, which had never been known to blossom, in garden pots, having heaped the mould as high as I could above the level of the pot, and planted the portion of the root nearly at the top of it. When the plants had grown a few inches high, they were secured to strong sticks, which had been fixed erect in the pots for that purpose, and the mould was then washed away from the base of their stems by a strong current of water. Each plant was now suspended in air, and had no communication with the soil in the pots, except by its fibrous roots, and as these are perfectly distinct organs from the runners which generate and feed the tuberous roots, I could readily prevent the formation of them. Efforts were soon made by every plant to generate runners and tuberous roots; but these were destroyed as soon as they became perceptible. An increased luxuriance of growth now became visible in every plant, numerous blossoms were emitted, and every blossom afforded fruit.

—afforded blossoms and fruits.

The redundant sap was made to afford tubers on the branches instead of the roots.

Conceiving, however, that a small part only of the true sap would be expended in the production of blossoms and seeds, I was anxious to discover what use nature would make of that which remained, and I therefore took effectual means to prevent the formation of tubers on any part of the plants, except the extremities of the lateral branches, those being the points most distant from the earth, in which the tubers

are naturally deposited. After an ineffective struggle of a few weeks, the plants became perfectly obedient to my wishes, and formed their tubers precisely in the places I had assigned them. Many of the joints of the plants during the experiment became enlarged and turgid; and I am much inclined to believe, that if I had totally prevented the formation of regular tubers, these joints would have acquired an organization capable of retaining life, and of affording plants in the succeeding spring.

Another experiment.

I had another variety of the potatoe, which grew with great luxuriance, and afforded many lateral branches; and just at that period, when I had ascertained the first commencing formation of the tubers beneath the soil, I nearly detached many of these lateral branches from the principal stems, letting them remain suspended by such a portion only of alburnous and cortical fibres and vessels as were sufficient to preserve life. In this position I conceived that if their leaves and stems contained any unemployed true sap, it could not readily find its way to the tuberous roots, its passage being obstructed by the rupture of the vessels, and by gravitation; and I had soon the pleasure to see that, instead of returning down the principal stem into the ground, it remained and formed small tubers at the base of the leaves of the depending branches.

Hence the tubers are formed by sap descending from the stems or leaves, through the bark.

The preceding facts are, I think, sufficient to prove that the fluid, from which the tuberous root of the potatoe, when growing beneath the soil, derives its component matter, exists previously either in the stems or leaves; and that it subsequently descends into the earth: and as the cortical vessels, during every period of the growth of the tuber, are filled with the true sap of the plant, and as these vessels extend into the runners, which carry nutriment to the tuber, and in other instances evidently convey the true sap downwards, there appears little reason to doubt that through these vessels the tuber is naturally fed.

Growth of the tubers impeded by intercepting the descending sap.

To ascertain, therefore, whether the tubers would continue to be fed when the passage of the true sap down the cortical vessels was interrupted, I removed a portion of bark of the width of five lines, and extending round the stems of several plants of the potatoe, close to the surface of the ground, soon after that period when the tubers were first

formed. The plants continued some time in health, and during that period the tubers continued to grow, deriving their nutriment, as I conclude, from the leaves, by an inverted action of the alburnous vessels. The tubers, however, by no means attained their natural size, partly owing to the declining health of the plant, and partly to the stagnation of a portion of the true sap above the decorticated space.

Probability that the sap descends through the alburnum where the plant is decorticated.

The fluid contained in the leaf has not, however, been proved, in any of the preceding experiments, to pass downwards through the decorticated space, and to be subsequently discharged into the bark below it: but I have proved with amputated branches of different species of trees, that the water which their leaves absorb, when immersed in that fluid, will be carried downwards by the alburnum, and conveyed into a portion of bark below the decorticated space; and that the insulated bark will be preserved alive and moist during several days;* and if the moisture absorbed by a leaf can be thus transferred, it appears extremely probable that the true sap will pass through the same channel. This power in the alburnum to carry fluids in different directions probably answers very important purposes in hot climates, where the dews are abundant and the soil very dry; for the moisture the dews afford may thus be conveyed to the extremities of the roots: and Hales has proved that the leaves absorb most when placed in humid air; and that the sap descends, either through the bark or alburnum, during the night.

This inverted action of the alburnum explains the facts in the experiments of Hales and Du Hamel.

If the inverted action of the alburnous vessels in the decorticated space be admitted, it is not difficult to explain the cause why some degree of growth takes place below such decorticated spaces on the stems of trees; and why a small portion of bark and wood is generated on the lower lip of the wound. A considerable portion of the descending true sap certainly stagnates above the wound, and of that which escapes into the bark below it, the greater part is probably carried towards, and into, the roots; where it preserves life,

* This experiment does not succeed till the leaf has attained its full growth and maturity, and the alburnum of the annual shoot its perfect organization.

and occasions some degree of growth to take place. But a small portion of that fluid will be carried upwards by capillary attraction, between the bark and the alburnum, exclusive of the immediate action of the latter substance, and the whole of this will stagnate the lower lip of the wound; where I conceive it generates the small portion of wood and bark, which Hales and Du Hamel have described.

Interesting facts respecting the annular decortication of the fir.

I should scarcely have thought an account of the preceding experiments worth sending to you, but that many of the conclusions I have drawn in my former memoirs appear, at first view, almost incompatible with the facts stated by Hales and Du Hamel, and that I had one fact to communicate relative to the effects produced by the stagnation of the descending sap of resinous trees, which appeared to lead to important consequences. I have in my possession a piece of a fir-tree, from which a portion of bark, extending round its whole stem, had been taken off several years before the tree was felled; and of this portion of wood one part grew above, and the other below, the decorticated space. Conceiving that, according to the theory I am endeavouring to support, the wood above the decorticated space ought to be much heavier than that below it, owing to the stagnation of the descending sap, I ascertained the specific gravity of both kinds, taking a wedge of each, as nearly of the same form as I could obtain, and I found the difference greatly more than I had anticipated, the specific gravity of the wood above the decorticated space being 0.590, and of that below only 0,491: and having steeped pieces of each, which weighed a hundred grains, during twelve hours in water, I found the latter had absorbed 69 grains, and the former only 51.

The wood above the decorticated space is much denser.

Whence considerable advantage may be derived in felling.

The increased solidity of the wood above the decorticated space, in this instance, must, I conceive have arisen from the stagnation of the true sap in its descent from the leaves; and therefore in felling firs, or other resinous trees, considerable advantages may be expected from stripping off a portion of their bark all round their trunks, close to the surface of the ground, about the end of May or beginning of June, in the summer preceding the autumn in which they are to be felled. For much of the resinous matter contained in the roots of these is probably carried up by the ascending sap in the

spring, and the return of a large portion of this matter to the roots would probably be prevented:* the timber I have, however, very little doubt would be much improved by standing a second year, and being then felled in the autumn; but some loss would be sustained owing to the slow growth of the trees in the second summer. The alburnum of other trees might probably be rendered more solid and durable by the same process: but the descending sap of these, being of a more fluid consistence than that of the resinous tribe, would escape through the decorticated space into the roots in much larger quantity.

The increased solidity is not confined merely to the vicinity of the decorticated space.

It may be suspected that the increased solidity of the wood in the fir-tree I have described was confined to the part adjacent to the decorticated space; but it has been long known to gardeners, that taking off a portion of bark round the branch of a fruit-tree occasions the production of much blossom on every part of that branch in the succeeding season. The blossom in this case probably owes its existence to a stagnation of the true sap extending to the extremities of the branch above the decorticated space; and it may therefore be expected that the alburnous matter of the trunk and branches of a resinous tree will be rendered more solid by a similar operation.

I send you two specimens of the fir-wood I have described, the one having been taken off above, and the other below, the decorticated space. The bark of the latter kind scarcely exceeded one-tenth of a line in thickness; the cause of which I propose to endeavour to explain in a future communication relative to the reproduction of bark.

* The roots of trees, though of much less diameter than their trunks and branches, probably contain much more alburnum and bark, because they are wholly without heart wood, and extend to a much greater length than the branches; and thence it may be suspected that when fir-trees are felled, their roots contain at least as much resinous matter, in a fluid moveable state, as their trunks and branches; though not so much as is contained, in a concrete state, in the heart wood of those.

VII.

The Invisible Lady; being an Explanation of the Manner in what the Experiment which was exhibited in London, by M. Charles and others, is performed. In a Letter from a Correspondent.

As the acoustic experiment of the Invisible Girl, which has excited so much attention and curiosity, does not appear to have been hitherto explained in any publication, I have sent you the following drawing and description of the manner in which it is performed. I must, however, in justice observe, that the conduct of this experiment has been kept in the most profound secrecy by the exhibitors, and that my information was obtained from the account given of it by Mr. Millington, in one of his philosophical lectures, last winter, in Chancery-lane; where I witnessed the experiment in its full effect; and by a comparison of his account with the exhibition which I have since visited, and find perfectly to agree with his description, I am fully convinced they are one and the same thing. If therefore you think the account I send you worthy of insertion in your valuable Journal, it is quite at your service, and it may perhaps afford information, and gratify the curiosity of some of your readers.

The experiment of the Invisible Girl has not yet been published.

It was explained by Mr. Millington in his lectures.

I am, Sir, your's, &c.

X.

Fig. 1, plate 2, represents a perspective view of all the visible apparatus of the Invisible Girl as you enter the room. It consists of a mahogany frame, not very unlike a bedstead, having four upright posts *a a a a*, about five feet high, at the corners, which are united by a cross rail near the top, *b b*, and two or more cross rails near the bottom, to strengthen the frame: these are about four feet in length. The frame thus constructed stands upon the floor, and from each top of the four pillars *a a a a* spring four strong bent brass wires, converging at the top *c*, where they are secured by a crown and other ornaments. From these four wires a hollow copper ball, of a foot in diameter, is suspended by slight ribbons so as to cut off all possible communication with the frame. This globe is supposed to contain the in-

The apparatus consists of four metallic trumpets connected with an hollow ball.

visible being, as the voice apparently proceeds from the interior of it; and for this purpose it is equipped with the mouths of four trumpets, placed round it in an horizontal direction, and at right angles to each other, as may be more distinctly seen at fig. 2, where *g* is the globe, *d d d d* the trumpets, and *b b b b* the frame surrounding them, being about half an inch from them. When a question is proposed, it is asked from any side of the frame, and spoken into one of the trumpets, and an answer immediately proceeds from all the trumpets, so loud as to be distinctly heard by an ear addressed to any of them, and yet so distant and feeble, that it appears as if coming from a very diminutive being. In this the whole of the experiment consists, and the variations are, that the answer may be returned in several languages, a kiss will be returned, the breath, while speaking, may be felt, and songs are sung either accompanied by the piano forte, &c.

—and the sounds seem to proceed from the trumpets.

But the sound is really conveyed by a tube.

After describing the manner in which this effect is brought about, it will immediately appear that the whole deception consists in a very trifling addition to the old and well-known mechanism of the Speaking Bust, which consists of a tube from the mouth of a bust, leading to a confederate in an adjoining room, and another tube to the same place, ending in the ear of the figure. By the last of these, a sound whispered to the ear of the bust is immediately carried to the confederate, who instantly returns an answer by the other tube, ending in the mouth of the figure, who seems to utter it: and the Invisible Girl only differs in this one circumstance, that an artificial echo is produced by means of the trumpets; and thus the sound no longer appears to proceed in its original direction, but it is completely reversed.

Description of the tube and other apparatus.

The apparatus necessary to produce this effect, is seen in fig. 3, where *bb* represent two of the legs of the frame, one of which, as well as half the hand-rail, is made into a tube, the end of which opens in the rail immediately opposite the center of the trumpet. This hole is very small, and concealed by reeds or other mouldings, and the other end communicates by a long tin half-inch pipe, *p p*, concealed under the boards of the floor, *f f*, and passing, concealed, up the wall of the room to a large deal case, *k*, almost similar to an inverted funnel, large enough to contain the confederate,

and a piano-forte. Any question asked into one of the trumpets, will be immediately reflected back to the orifice of the tube, and distinctly heard by a person in the funnel, and the answer uttered by them, or a song or tune from the piano-forte will be distinctly heard at the mouths of the trumpets, but no where else, and there it will seem to come precisely from the interior of the globe. A small hole closed with glass is left through the funnel and side wall of the room, as at *w*, by means of which the concealed person has an opportunity of observing and commenting upon any circumstance which may take place in the room.

VIII.

Mr. William Russel, *of Newman Street, has offered Proposals for publishing, by Subscription, two Engravings of the* Moon in Plano. *By the late* John Russel, *Esq. R.A. with the following adress.*

THE late Mr. Russel, celebrated amongst men of science for the production of the Lunar Globe, left, at his death, two Lunar Planispheric Drawings, the result of numberless telescopic observations scrupulously measured by a micrometer: one of which Drawings exhibits the Lunar Disk in a state of direct opposition to the sun, when the eminences and depressions are *undetermined*, and every intricate part, arising from colour, form, or inexplicable causes, is surprisingly developed and exquisitely delineated; and the other, of precisely the same proportion, represents the eminences and depressions of the moon *determined* as to their form with the utmost accuracy, producing their shadows when the sun is only a few degrees above the horizon of each part. The former of these was beautifully and most correctly engraved by Mr. Russell, who had likewise very considerably advanced in the engraving of the latter, when death terminated his labours: it is, however, left in such a forward state, that it will be finished with the greatest exactness, and all possible dispatch.

Mr. William Russell, son of the late Mr. Russell, proposes to publish, by subscription, these lunar plates, which have been

long promised to the scientific world: and the first engraving is now offered for their inspection. The whole will be incomparably the most complete lunar work ever offered in any age —a work, the more carefully it is examined, either as to its accuracy or elegance (effected indeed, by *extreme* labour during twenty-one years), the more it will excite the wonder and admiration of the diligent inquirer.

The utility of these engravings is best expressed in the author's own words: "The principal use of the moon to astronomers, is, that of ascertaining the longitude of places by the transit of the earth's shadow, when the moon is eclipsed. The shadow of the earth coming in contact with many known spots, if the observation be made in different places at the same time, the longiaude of each place could by this means be ascertained with great precision, provided the spots to be made choice of be sufficiently represented and recognised; but there being *no* faithful delineation of the moon, and the *edges* of those spots which are known being undefined, the observations made have not been so useful as could be wished: for this purpose, it is believed, Mr. Russell's labours will be found very useful, and will very much add to the certainty and precision of the observations on Lunar elipses; as the chief design of his planisphere, representing the moon in a state of opposition to the sun, is directed to this end, and which he has spared no pains in bringing to perfection."

These engravings, it is expected, will not only prove of great utility to the astronomer, but lead to very important speculations in natural philosophy. The remarkable changes of forms in various eminences, the different radiations of light observable at one age of the moon and not at another, with its numerous surprising pheriomena, are in these plates faithfully and fully expressed, so as to form a work, it is presumed, highly interesting in the departments either of Astronomy or Natural Philosophy.

CONDITIONS.

1. The diameter of each Planisphere is fifteen inches.—2. The impressions shall be delivered in the order of subscribing.—3. The price to subscribers for the work is five guineas: an advance will be made to non-subscribers, when the whole has been completed,.—4 One half of the above sum to be paid

at the time of subscribing; when one part of the work will also be delivered.—5. The description, &c. of both plates will be given when the second plate is paid for and delivered.—6. The whole of the printing shall be executed by the first printers, upon the best wove paper,

Subscriptions are also received at Mr. FADEN's, Geographer to his Majesty, and to His Royal Highness the Prince of Wales, Charing Cross.

Sir JOSEPH BANKS, K. B. Sir H. ENGLEFIELD, Drs. MASKELYNE, MILNER, HERSCHEL, HON. H. CAVENDISH, Earl of EGREMONT, are amongst the names already subscribed.

VIII.

Letter of Inquiry from a Correspondent, whether the Light and Heat Company is entitled to public Encouragement.

To Mr. NICHOLSON.

SIR,

I SEND you a collection of papers which have been circulated by Mr. Winsor, patentee for lighting apartments by the gas from pit-coal, who is soliciting an immense subscription, in order to establish a public company. On former occasions, I observe that you have not hesitated to give your opinion without reserve, upon subjects by which the public might be benefited. I trust you will suffer the same motives to operate on the present occasion.

Letter of inquiry respecting Mr. Winsor's Light and Heat Company.

I am, Sir,

Your obliged and constant reader,

M. P.

REPLY. W. N.

THOUGH I had heard of the scheme in question some time ago, I did not find sufficient motives for paying any parti-

cular attention to it, until the papers were sent me by my correspondent, and they now come so late, that I can treat the subject only in a cursory manner. The following remarks will require no apology to my readers for the freedom with which they are made.

The invention was publicly known, long before Mr. Winsor had his patent.

I. To give light by the gas from coal was many years ago done by Lord Dundonald. It was afterwards shewn in public, anno 1784, by Diller and others. Mr. Murdoch (see Philos. Journal, XI. 74. for May 1805) extensively applied this practice in Cornwall in 1792, and afterwards at Soho in 1798, and since. And many years afterwards, Mr. Winsor takes out a patent for this very object. He is not the *first inventor as to the public use and exercise thereof*, and therefore *his patent is void* by the statute of James I.

If Mr. Winsor's patent were for a new invention, he renders it void, by sharing it to more than five.

II. Mr. Winsor, in his paper intitled *Terms and Conditions, &c.* says his patent is vested in four respectable gentlemen, whose names, for the respectability of his project, he ought to have given as well as his own. He proposes, that, *instead of the co-patentees engrossing the whole of their patent privileges to themselves, they will share them with a large and respectable number of their countrymen.* Now as the Letters Patent are merely grants of privileges, it cannot be questioned but that every one who shall be admitted to any share or interest in the privileges so granted, will become a joint patentee; and of course, that if that number exceed five, the patent itself will be rendered void, by virtue of the proviso therein contained for that purpose. The paper here first mentioned is the basis of subscription, and does certainly give an equitable interest in the patent to every one who subscribes, as far as the number five; and after the admission of the sixth subscriber, the patent itself, even if not otherwise exceptionable, becomes a nullity.

There is no probability of his obtaining an act of parliament.

III. Mr. Winsor solicits subscriptions, on the condition that a company shall be established by an act of the legislature, —as if the legislature were at his command, or as if an act of parliament could be had as a thing of course. I do not think it needful to discuss his calculations of profits, and the statistic inferences he pretends to draw from them. Much as they are open to objection, I would only ask, whether the legislature is likely to consent that he and his subscribers shall levy upon the nation an annual interest of 575l. for every 5l. subscrip-

tion, to the amount of many millions—even supposing his merits as an inventor to be as much above par as my first paragraph appears to place them below it?

IV. Lastly, as a matter of prudence between man and man, I would ask who are the responsible trustees for the subscriptions, which, at five pounds each, would amount to one hundred thousand pounds? I am very far from inviting a discussion of any man's private character unnecessarily, and of Mr. Winsor I know absolutely nothing: but I must say, that in a concern of much less apparent magnitude than the present, common sense and common integrity ought to have dictated the insertion of the names of trustees in the printed papers before me. Two respectable banking-houses are indeed named for receiving subscriptions, and I should hope, for their credit as honourable men, that they have consented to be bankers to persons who are known and recommended to them as fair and proper connections, more especially in a project of such apparent moment and doubtful import. I dare not presume the contrary; and all that I have to say on this head is, that it would have been no more than simple justice and open dealing, if they had insisted that the public should also have known where the powers may be placed of drawing for the monies in their hands, and disposing of the whole at the pleasure or discretion of such drawer.

Neither the partners nor trustees of Mr. Winsor are known.

—but they ought to have been declared.

It is hoped that the bankers who know the project, do also know the trustees.

IX.

Observations of Dr. Carradori, showing that Water is not deprived of its Oxygen by boiling.

MESSRS. Humboldt and Gay-Lusac, in an interesting memoir presented to the National Institute, and entitled, "Experiments on Eudiometric Methods, and the proportion of the constitutuent principles of the atmosphere, &c.," are, as it appears, of opinion, that ebullition is the most effectual means of depriving water of oxygen. In effect, they have availed themselves of this operation only for obtaining this

end; and they afterwards assert, that where water is gradually heated, the proportion of oxygen encreases as the heat approaches to ebullition; whence they have concluded, that in the degree of the heat constituting the temperature of ebullition, the oxygen is most easily driven off, and that there is no other power for disengaging it. But we find, by experience, that ebullition is not sufficient to divest the water of all the oxygen it contains, whether attached or combined. Ebullition deprives water of much of the oxygen and other gas with which it is impregnated, but it cannot entirely separate them; for it is proved that water well boiled always retains oxygen. Nothing but congelation, and the respiration of fishes, can clear water entirely of its oxygen: these two are the only means that complete the separation from water of all oxygen it contains interposed between its globules; for it is not till then that we obtain an exact proof of its being divested of it. As to the rest, the detaching and decomposing power of heat, at the degree of ebullition, is not sufficient to overcome the affinity and attractive power of all the oxygen united with the water; a part of it remains obstinately fixed, in spite of all the heat.

Fishes, as I have elsewhere observed, are the eudiometers of water, and one of those, shut up in a body of water, is capable of separating, by means of its respiration, in several hours, all the oxygen from the water, and to exhaust it entirely of this principle. It is by this method that boiled water is proved to be not entirely divested of oxygen, but still contains it.

If we take a quantity of water, and boil it for any length of time, and then pour it quite boiling into a bottle, or a glass vessel with a narrow neck, so that it be full up to the top; if a portion of oil be poured upon it to prevent the air from penetrating, and it be then suffered to cool: in this state, let the oil be removed, and a little fish thrown in, and the oil be immediately replaced, the fish will continue to live some time in this water, and will be seen to breathe.

Ebullition, therefore, has not removed all the oxygen of the water; but a portion of it remains sufficient to serve for the respiration of the fish; for when the water is really deprived of all its oxygen, fishes thrown into it die instantly.

because they cannot breathe. This is a matter of fact that any one can verify.

But again, let us take snow, and introduce it by little and little into a glass bottle, continuing to do so in proportion as it melts and lessens in bulk, till, being entirely melted, it shall fill the bottle with water up to the brim; let oil be immediately poured on the surface, so that it shall rise some inches in the neck of the bottle; let us then permit it to acquire the temperature of the atmosphere, which I suppose to be warm, and capable of melting the snow; if, in this case, by some expedient, the oil placed on the snow water, and which embarrasses the neck of the bottle, be drawn off, and we introduce, with the greatest possible quickness, a fish, as vigorous as possible, and cover the water immediately over with oil, we shall see with what pain this animal is affected in this water; he is attacked with a mortal convulsion, and in a little time ceases to live.

Such water as is obtained from melted snow, is equally obtained from ice or from hail, by introducing pounded ice or hail, by little and little, into a bottle, with the attentions here mentioned; and by throwing in a fish, after these matters are melted, the animal dies in this as in the snow water.

Thus we clearly see that congelation expels from water all the oxygen it contains, and on that account fishes cannot live, because they cannot breathe in it. Water of snow, of ice, or of hail, or water produced from any congealed mass, under whatever form it may be, is a mortal element for the inhabitants of that fluid, because they find it void of oxygen gas, which stops their respiration. These waters are exhausted of oxygen, like that which has served for the respiration of one of these animals until its death.

If a bottle be filled with any kind of water, that is to say, of river, of well, or of spring water, and a little fish be put into it; and afterwards to hinder the water from the absorbing oxygen of the atmosphere, if oil be poured into the neck of the bottle upon the water, the fish will live many hours; but after he shall, by respiration, have exhausted all the oxygen, he will die. Let another similar fish be then thrown into it, he will die as soon as he is in this water. But if it be wished that the water should again become fit for

maintaining the life of fishes, this may be instantly effected by pouring it into a large vessel, where it can again absorb the oxygen of the atmosphere. The same observation is true with regard to the snow water; we may render it capable of supporting the lives of fishes, and of serving for their respiration, if we put it in a vessel that shall expose a large surface to the air, in order that it may again absorb the oxygen it has thrown off during the congelation.

Snow or ice water is then, without any doubt, destitute of oxigen, as well as that which has served for the respiration of fishes, who have the faculty, by this process, of separating and of absorbing all the oxygen, which is there in a state of solution.

There is not the slightest difference between snow and ice water, with regard to the privation of their oxygen; both are divested of it. So that what those two respectable physicians have advanced in their memoir, does not appear to be well founded. It appears, according to them, that ice contains a portion of oxygen, but that water in congealing throws off a great part of it, mixed with azotic gas, and that water in its transformation into snow throws off less air than when transformed into ice; because, when they caused snow newly fallen to melt, by gradual warmth, they have obtained from it a mass of air almost double that afforded by melted ice.

It is true we see much air disengage itself from snow while melting: but it is not any air contained in the frozen or chrystalized water which constitutes the snow; but it is an air confined between the interstices of the snow, remaining attached to the faces or surfaces of the chrystals that compose it; and it is for this reason that we see much air proceed from the snow while melting. I have already stated, many of those observations in two memoirs on snow water, inserted in the Journal de Physique of Paris, of Ventose, in the year 7, and of Thermidor, in the year 9; for which reason I shall not extend these reflections to any greater length.

SCIENTIFIC NEWS.

Royal Society.

THE Bakerian Lecture has been lately read before the Royal Society by H. Davy, Esq. F.R.S., on Electricity considered as to its chemical agencies. Mr. Davy has found that a great number of bodies are capable of being decomposed by electricity; particularly those containing alkalies, acids, alkaline-earths, and metallic oxides: and he finds that their elements are separated in a voltaic circuit, made with water, in such a way, " that all acid matter arranges itself round the positively electrified metallic point, and all alkaline matter, and the oxides, round the negatively electrified point. In this way, he decomposes insoluble as well as soluble compounds. Sulphuric acid and the earths are separately procured from the earthy neutral compounds, and soda and potash evolved from minerals and stones containing them,

Chemical agency of electricity.

The voltaic current separates acids from their bases; so that the acids arrange themselves round the positive point, and the bases round the negative.

By the attracting and repellent powers of the different electricities, acid and alkaline matter are transported through menstrua for which they have a strong attraction, and through animal and vegetable substances, Thus sulphuric acid will be repelled through an alkaline solution from the negative to positive point, and *vice versa;* potash or lime will be repelled from the positive point to the negative, through an acid solution.

This power is so strong, that it will prevent the usual effects of chemical affinity.

Mr. Davy explains these phenomena by means of some other experiments. As in " Voltas contacts of metals, copper and zinc appear in opposite states; so Mr. Davy finds that acids and alkalies, with regard to each other, and the metals, possess naturally the power of affording electricities," and may be said to be respectively in states of negative and positive electrical energies; and the bodies naturally negative are repelled by negative electricity, and the bodies naturally positive attracted by negatively-electrified points.

Mr. D.'s explanation of these effects is, that he finds the acids and the bases to possess respectively the negative and positive states, and consequently are attracted by the contrary states.

Question: whether chemical affinity and electric energy be not the same power?

As chemical affinity is modified, destroyed, or increased, by modifying, destroying, or increasing the natural electrical states of bodies, and as all bodies that combine chemically, which have been accurately examined as to their electricities, are in states of opposite energy, Mr. Davy puts the question, "Whether chemical unions and decompositions are not the result of the electrical energies of the bodies? and whether elective affinity is not the same property with electrical energy?" He enters into various illustrations and applications of this theory, which naturally arises from the facts.

These general principles explain many galvanic and chemical facts, and afford new methods of analysis.

The general principles explain a number of phænomena before obscure: why acid and alkali were obtained from water apparently pure; the acid and alkaline bases produced by the different poles of the pile; the decomposition of muriate of soda between the plates; the separation of water into oxygen and hydrogen, by attracting and repellent powers acting equally upon other bodies.

The experiments offer new methods of analysis, and will apply to the solution of many natural phænomena.

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A

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OF

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AND

THE ARTS.

FEBRUARY, 1807.

ARTICLE I.

Account of a Fact, not hitherto observed, that the Galvanic Power heats Water while decomposing it in Part. In a Letter from Mr. John Tatum, Jun.

To Mr. Nicholson.

Sir,

In the various galvanic communications which I have had the pleasure of consulting in your invaluable Journal, as well as in lectures and volumes on that interesting subject, to which I have attended, I do not recollect any mention being made on a circumstance attending the decomposition of water, which I observed about two months ago, in preparing for a public lecture I was about to deliver, which, if you think worthy a place in your Journal, is very much at your service. Circumstance not hitherto noticed in the galvanic decomposition of water.

In the experiment alluded to I made use of four troughs of the following dimensions, viz. two of 26 plates, each plate 50 inches, and two troughs of 25 plates, each plate 36 inches, Experiment. Water was decomposed by troughs of considerable surface.

inches, of course I had the power of 4400 inches. The oxidating fluid I made use of was good nitrous acid, with about sixteen times its bulk of water. On passing the galvanic fluid through about one ounce of water, by means of platina wires, I was much surprised at the quantity of caloric which was liberated as the water became decomposed; the temperature of the tube which contained the water seemed to be about (as near as I could judge by the touch) 180° of Fahrenheit; but as I wished to ascertain the temperature more correctly, I immediately applied the bulb of a thermometer, though it was very unfavourable to ascertain the temperature, as the bulb of a small thermometer could touch a tube of one inch diameter at a very small surface, but the mercury soon rose 10°. Not having any person with me, and endeavouring to regulate the wire which communicated from the battery to the tube of water with one hand, while the other was engaged in holding the thermometer against the tube, I accidentally brought the positive and negative platina wires in contact, which exploded the gases, and forced the tube violently up a considerable height, which falling on the table, broke, and prevented me accomplishing my wish relative to the temperature of the water under decomposition.

Much heat was produced in the water.

The thermometer shewed 10 deg.

Another experiment.

A short time after, in my lecture on the subject, I noticed the circumstance; and one or two of my audience, after the lecture, on applying their hands to the tube while the water was decomposing, hastily withdrew them on account of the heat.

How was the heat produced?

I cannot, Sir, account for the liberated caloric by any other means than supposing that the galvanic fluid furnished more caloric than was necessary to convert the water into gases. If, Sir, this meet with an insertion, I shall communicate more on the above subject the first opportunity; until when permit me to remain,

Your's, most respectfully,

JOHN TATUM, Jun.

Dorset Street,
Jan. 6th, 1807.

II.

Account of the Discovery of the Means of illuminating by the Gas from Coal, by Dr. Clayton, *previous to the Year* 1664. *In a Letter from Mr.* John Webster.

To Mr. Nicholson.

Sir,

I READ with great pleasure your just observations on Mr. Winsor's gaseous proposals for enlightening the inhabitants of this metropolis. The purport of these few lines is merely to say, that the discovery of the carbonated hydrogen gas took place previous to the year 1664. Carbonated hidrogen long ago discovered

Happening a short time ago to be reading some of Boyle's manuscripts, in the British Museum, I met with a paper of the following title (Ascough's Manu. 4437).

"Experiments concerning the Spirit of Coals, in a letter to the Hon. Mr. Boyle, by the late Rev. Jas. Clayton, D.D. B. Mus." —by Dr. Clayton.

As I did not copy the whole of the paper, I have taken the liberty of sending you the few short notes I made at that time.

The experiments were undertaken by Dr. Clayton, in consequence of having discovered that gas, issuing from fissures near a coal-pit at Wigan, in Lancashire, ignited when a burning candle was presented to it. History of the discovery.

Dr. Clayton on observing this effect distilled coal. He first observed that "Fleghm" came over, afterwards a "black oyle," and then an "inflamable spirit." He collected the last product into bladders, and amused his friends by pricking a hole in a full bladder and igniting the gas. The Doctor distilled coal, and received the gas in a bladder.

If you think it worth while to insert this in your valuable Journal, it may be the means of gratifying some of your readers by referring them to the original paper, the number of which I have already put down.

From your obliged humble servant,

Jan. 5, 1807. JOHN WEBSTER.

III.

Observations on the Metallic Co...position of the Specula of reflecting Telescopes, and the manner of casting them; also a method of communicating to them any particular Conoidal Figure; with an attempt to explain, on scientific Principles, the Grounds of each Process, and occasional Remarks on the Construction of Telescopes. *By the Rev.* JAMES LITTLE*.

[Concluded from p. 59.]

The polisher must be of pitch, rather soft.

THE consistence of the pitch is, in this business, an article of the first importance. Soft pitch will give to the polish a higher lustre, and will less expose the face of the mirror to scratches: but, if it be too soft, the mirror will sink in it, like a seal in soft wax; and the figure of the polisher cannot be preserved, nor the furrows in it from being defaced. It must, therefore, be always harder than common pitch is, in a mean temperature of the air in this climate. And, after the polishing powder is bedded in it (which must at first be laid on so copiously, that the pitch may not rise up to the surface, between the particles of it,) and when the mirror has been worked on it a little time, then all the loose particles of the powder ought to be washed off, from the edges and furrows of the pitch, with a sponge or brush, (made of fine hair,) under water, that no grains may get on the surface, and injure the polish. And, if this be attended to, and the pitch be a little softened by heat, when the powder is first applied, it may be used of a consistence hard enough, without inconvenience: but, if it be made so hard, as not to sink at all, or expand itself, under the mirror, I believe it will never communicate to it a perfect figure.

The larger the polisher the flatter the figure.

From what has been here laid down, it must be obvious, that, by diminishing the size of the polisher, whether it be of a circular or elliptic shape, the curvature of the mirror will be brought nearer to that of a circle; and, by enlarging the polisher, the curvature will approach to that of an hy-

* Irish Transactions, Vol. X.

perbola,

perbola, when the precautions here given are observed. Both these may be done, by spreading the pitch on the polisher, to a greater or lesser extent.

In the Gregorian telescope, the excess of curvature, in the great mirror, may be remedied, by a defect of it in the little mirror, and *vice versâ*. It must be desirable, to a fabricator of this instrument, to understand why this is so; and how a change in the curvature may be effected: for an artist cannot well execute a project, the design of which is to him unknown; nor improve by trials, even repeated, if they are made in the dark. I apprehend, that, in this kind of telescope, the mirrors are commonly selected, out of a number finished of each size, as they happen to suit each other: and, if there should be but few pairs in the assortment, whose irregularities compensate one another, few good telescopes will be produced. This would be less frequently the case, and the Gregorian telescope be more improved, if a more certain method were known, of giving, to each pair, their appropriate figure at first, or of altering it in either, where it is defective. Perhaps persons, not much versed in optics or geometry, may be assisted, in discovering the evil, and the remedy, from the following remarks; which are given in words, in order to dispense with a diagram.

The defects of the large speculum may be compensated by the figure of the smaller.

The curvature of the circumference of a circle is uniform in every part, being (in an arch of it, of a given length) so much the greater, as the radius is smaller, and *vice versâ*. But the curvature of the ellipse, parabola, and hyperbola, is not uniform, but continually diminishes, from the vertex of these curves, (which answers, in the present case, to the center of the mirror,) to the extremity on each side; but it diminishes less in the ellipsis than the parabola; and in this than in the hyperbola. So that, if we suppose a bow to be bent, at first, into an arch of a circle, and, when gradually relaxed, to become, towards its extremities, more and more straitened, as it unbends, while the curvature, at the very middle, remains the same, it will successively form these three curves, in the above order. And, if concave mirrors had the same curvature with them, they would have the following properties.

Popular observations on the conic sections.

If the speculum be of a parabolic form, rays of light, falling on it, parallel to its axis, or issuing from a luminous point

The parabolic speculum is good for the

Newtonian or Gregorian telescope.

point in the same, so very distant, that they may be regarded as parallel, will converge, by reflection, to one point in the axis; which point is the focus. The same is nearly true of rays coming from luminous points not far from the axis, or lying in a very contracted field of view, so as to make but a very small angle with the axis: the rays, coming from each single distinct point in the object, are converged to so many single distinct points in the image, formed at the focus of the mirror. Hence, the excellence of a parabolic mirror, for the larger speculum of the Newtonian or Gregorian telescope*.

If

* But, because a parabolic mirror reflects, to one point, rays, that fall on it, parallel to its axis, it follows, that it will not converge, to a point, rays, that are diverging or inclined to its axis. The former, (if the point, from whence they radiate, be in the axis of the mirror,) would be reflected from any line, drawn diametrically across the mirror, in a caustic curve double and cuspidated: the latter, (being in the same plane in which is the radiant point, infinitely distant, and the axis,) would form a curve nodated. So that the excellence of a parabolic mirror is for viewing remote, but not near objects. And a person might thus be deceived, who would judge of the goodness of a telescope, only from its rendering print legible, at a small distance, from whence the breadth of the great mirror would subtend an angle of sensible magnitude: for the pencils of rays that issue, diverging from each point of the printed letters, will be reflected, by the central part of the mirror, to a focus nearest to it; and the rays of each pencil, that fall on the exterior annuli of the mirror, will be reflected to points more remote from it. So that if, in the Newtonian and Gregorian telescope, the great mirror were of the correct figure of a parabola, and the little mirror, of the latter, were that of an ellipsis; and, if either telescope were adjusted to distinct vision, when the innermost zone only of the great speculum is exposed to the light; yet, the object would be indistinct, if seen by the rays reflected from the outer zone, unless the little mirror were removed farther from the great one. Hence, a spherical mirror is better than a parabolic one, for viewing very near objects; and neither of them can be equally adapted for viewing these and very remote objects. The distinctness of the telescope is, therefore, best proved, by directing it to the stars: if it shews, clearly, the fasciæ, on the disk of the planet Jupiter, or the ring of Saturn, it will deserve to be approved of. I have ground and polished, in the manner here described, the mirrors of a little Gregorian telescope, of nine inches focus, which shewed these objects most distinctly; and I could not afterward, with much greater pains, execute another one, (neither indeed did I ever see one,) of that size, of equal accuracy: which served to convince me, of the exquisite correctness required in the

If the mirror be of an elliptic, or oval, curvature, rays, issuing from single points, in, or extremely near to one of its foci, and falling on it, (such as the rays proceeding from the single points in the image, formed by the parabolic larger syeculum,) will be converged to so many single distinct points, in the other focus of the elliptic mirror. Hence, the excellence of an elliptic figure, for the smaller speculum of the Gregorian telescope*. But the case is very different, The small speculum ought to be elliptical.

the figure of the mirrors, and of how great perfection the iostrument is susceptible. Telescopes have been recommended, for their enabling persoos to read gilded letters, at a considerable distance; hut this is an improper method for determining their merits; for (beside the ground of error now mentioned) a moch greater quantity of light is reflected from gilt letters, than from those of commoo print oo paper.

* But this will not be the case, if the rays diverge from points, so remote from the axis of the speculum, as to make a considerable angle with it, aod to fall very obliquely on the speculum: which would be the case, if the field of the telescope were too large, or if the focus of the great specnlum were too long, with respect to that of the lesser one: because, io either case, the image, formed by the great mirror, which is the object, with regard to the lesser, will have too great latitude; and the extreme peocils, diverging from it, fall, with too much obliquity, on the latter, to be collected by it, to single points, in the second image. And, on this accouot, there is, in the Gregorian telescope, a limit set to the degree of magnifying, so far as this depends on the mirrors, be their figure ever so correct. And, if any aberration prevail, in the image formed by the larger concave, they will be magnified by the lesser, were it perfect, in the proportion of the focus of the former to that of the latter. I am of opioion, that it is better not to aim at a high degree of magnifyiog, by the little mirror of this telescope; but, to endeavour to secure the correctness of the second image; and to lay the chief stress of the amplificatioo (as it is in the Newtonian telescope) on the eye glasses; because of the above circumstance, which no correctness, nor compensatioo of the mirrors, can remedy. From this inconvenience, here stated, the Newtonian telescope (the most perfect of all the constructions, that ever were or ever will be devised) is entirely free. This the author effected, by puttiog the eye-glasses on a differeot axis from that of the mirrors; by which he was enabled to make the lesser mirror a plane surface. Aod it will appear, oo due consideration, that he was obliged to iotroduce this change, in Gregory's telescope, of oecessity; aod oot from a low ambition, to which his mind was superior, that of obtrodiog his own inventions, to supplant those of equal merit by other men: though he has not stated all the imperfections of the Gregoriao form, nor the advaotages of his own; having only, in answer to objections, and, as it were, reluctantly, mentioned the chief circumstances, justifying the alteration he had recommeoded. It is adviseable that the magnifyiog power of the Gregoriao telescope should be chiefly dependeot oo the eye-piece.

in

in a spherical or hyperbolic mirror. From either of these, the rays, which issue in a cone or pencil, from single, luminous, distinct points, in a very remote object, and fall on them, will not converge again, to so many single points; but will, in the mean focus, of the mirror, be dispersed, and blended together in a small degree, yet sufficient to produce an universal haziness and indistinctness, over the whole surface of the object viewed in a telescope, having its large mirror of these forms, because it occurs, with respect to every point in such object; of which the following are the circumstances.

Effects of spherical mirrors.

If the mirror be spherical, those rays, nearly parallel of each pencil, which fall on it, next to its centre, will converge to a point more distant from the mirror, than the focus of any rays, that fall between the centre and outer edge of the mirror. And those, that fall on the outer extremity of it, will converge to a different point, nearest to the mirror: and the rays, which are incident on the several concentrical annuli, indefinitely narrow, of which the face of the mirror is composed, will have an indefinite number of points of convergence; each annulus its own point, and all lying in a series, in the axis of the pencil, between the points, or foci, of the extreme, and of the innermost annulus *. So that no entire incident pencil will, after reflection, converge to one point, unless the radiant point were in the centre of curvature of the mirror.

Contrary aberration of the hyperbolic mirror.

The property of an hyperbolic mirror is of the same nature, but with effects reversed: for, in this, the rays parallel to its axis, which are incident on its outer annulus, will converge to a point the most distant from it; and the rays, falling on its innermost annulus, will have their focus the nearest to it. And this is easy to comprehend: for, as the curvature of the hyperbola continually diminishes from its vertex, on each side, a parallel, or diverging pencil, falling

* This property of a spherical mirror has never, so far as I know, been synthetically demonstrated, by any optic writer, though it is a fundamental theorem in catoptrics. Mr. Robins derisively objected to Dr. Smith, that he had not demonstrated it. The Doctor, I believe, might have retorted the same charge on Mr. Robins. I have some reason to think, it is difficult to give such a demonstration of it, and that it will reflect credit on the person who furnishes it.

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at a distance from the vertex, on a mirror of this form, must (as in the case of a mirror of greater radius, i. e. of less curvature) have its focus formed farther from it, than if it were incident near the middle or vertex, where the curvature of the mirror is that of a circle of lesser radius.

These errors are not corrected by the small speculum.

And thus it is evident, that, as the several pencils, reflected by the great mirror, when it is spherical or hyperbolical, do not converge, each to a single point, but to a series of points, whose length is the depth of the focus of the mirror; so, neither do these pencils, in proceeding on to the little mirror, diverge each from a single point, but from the same series of points. So that, though the little mirror were formed truly elliptical, it would not make each of these pencils converge again (at the place of the second image, formed behind the first eye-glass) to single points, but to another series of points; by which the rays of contiguous pencils would be blended with one another, and make the object which is viewed, by means of these pencils, so transmitted to the eye, and, by it, refracted to a third series of points, near the retina, at the bottom of the eye, appear hazy and indistinct.

Inductions.

These remarks will be applied to our present purpose, by considering:

First. That the rays, reflected by the several annuli, in the surface of the great mirror, will fall on the annuli of the same order, in the little mirror; the rays from the outer, inner, or intermediate annuli of the one, proceeding to the like annuli in the other.

Secondly. That the farther the focus, or point of convergence, of any annulus of the great mirror, is distant from that mirror, the nearer will be the point of divergence of this part of the whole pencil, (among the series of such points), to the little mirror. Also, that the interval, between any one point in the series and this mirror, cannot be altered, by moving the mirror, without altering the intervals of all the rest; which, after the telescope is brought to the distinctest vision, cannot be permitted.

Rationale of the figure of the small mirror.

Thirdly. That, if the focus of any annulus, of the great mirror, be farther from it, than those of the other annuli, and, consequently, nearer to the little mirror than those; the rays, issuing from it, to this mirror, will not be reflected

by

by it, to the same point with those of the other annuli, unless the curvature of the corresponding annulus, of the little mirror, be increased, in the proportion of its radius to that of the great mirror; for, then only will the focus of rays, reflected by this annulus of the little mirror, be shortened, as much as, by the effect of that of the same annulus, of the great mirror, it would otherwise be lengthened. The same is true, *vice versâ*, if the focus of any annulus, of the great mirror, be shorter than those of the other annuli.

Fourthly. That, if there be any excess or defect, in the curvature of the great mirror, from that of a parabola, (and, consequently, a contraction, or elongation, of the foci, of the extreme rays of the reflected pencils,) there is no remedy, while this remains, but to make the little mirror so much deficient, or excessive in curvature, from that of an ellipse, (and, consequently, to lengthen or contract the foci of the extreme rays of the pencils reflected by it,) as its focus is, in proportion to the focus of the great mirror; there being no other means of reducing all the rays, of each pencil, to one point, at the second or conjugate focus of the little mirror; by which alone, the second image, consisting of such points, can be formed, and viewed distinctly, through the last eye-glass.

From all which, it is manifest, that, if the curvature of the great mirror be hyperbolical or deficient, then that of the little mirror ought to be spherical or excessive; and if the great mirror be spherical, the other must be parabolical or hyperbolical, according as its focus is long or short, in respect of that of the great mirror.

Instructions for examining the adaptation of the mirrors to each other.

Should the telescope be faulty, from indistinctness of vision, it may be corrected, by altering the figure of either of the mirrors, as shall be most practicable. And, to know what the alteration should be, the method. directed by Mr. Mudge, may be followed, of excluding the light from the central, middle, or extreme zone of the great mirror, by fixing, on the mouth of the tube, three annular diaphragms of pasteboard, &c. answering, in size and shape, to these zones respectively; by removing any of which diaphragms, the light will be admitted to the corresponding part of the mirror. If, then, by help of the adjusting screw, the object be first viewed distinctly, when the inner or central zone,

zone, only, of the mirror is uncovered to the light; and it be necessary, afterward, when it is seen by means of the exterior zone only, to remove the little mirror farther from the great one, (by turning back the adjusting screw,) in order to distinct vision; then one, or both of the mirrors, is deficient in curvature, i. e. the great one is hyperbolical, or the small one parabolical. And, on the contrary, if it be necessary, in this process, to bring the little mirror nearer to the great one; then one or both of the mirrors is spherical. For, in the former case, it is plain, that the mean focus of the outer zone of the little mirror is nearer to the second eye-glass, than that of the inner zone; since it is necessary to withdraw that focus, by putting back the little mirror: and the contrary is evident, in the latter case. The former could happen, only by the focus of the extreme rays, of each single pencil, being too far from the great speculum, (i. e. from its being hyperbolical,) and too near to the little one; or from the latter being deficient in curvature, near its edges; and thus throwing the focus of the rays, that fall there, too far from it, and too near to the last eye-glass. The second effect could arise only from a figure of the mirrors, the reverse of this. In the Newtonian telescope, there can be no doubt, where the defect of curvature is, because it has but one concave mirror.

How to correct the curvatures

When it has been thus determined what the defect is, means must be employed to correct it; and it may be expected, that, unless some certain mode, of effecting a different curvature of the great mirror, from that of the little one, is discovered, and skilfully practised, there will be but few good telescopes, of the Gregorian form, constructed. For, if both mirrors be polished, in the same manner and method, it is likely, that the defects in their figure, and the species of their curvature, will be the same in both. Whereas, it has been shewn, that all these ought to be directly contrary in one, from what they are in the other; referring to the parabola and ellipse, as the standard degrees of curvature.

—by means of the polisher, without grinding anew.

Now, the circumstances, which, in the method of polishing above-mentioned, have a tendency to produce particular species of conoids, have been already explained, and need not be repeated. But, as to the means of altering any figure

already given, to the great mirror, in the Newtonian telescope, or to either of the mirrors in the Gregorian, which happens to be unsuitable to the other one; I have to observe, that, in my trials, I have found this could be effected on the polisher, without putting the metal to be ground again upon the hones. For if it has, at first, been formed to a tolerably correct figure, of any species, then a very small reduction, of the substance of the metal, will produce a sufficient alteration of its form. If the change required consists in a diminution of curvature, a continuation of the process, under the regulation before-mentioned, will, without any alteration of the polisher, generally, be sufficient to produce it, from the degree of curvature of a circle, to that of the ellipse, parabola, or hyperbola, in order; or from any of these, to the others, in succession*. But, if the degree of curvature, already given, is to be increased, and to verge more toward the circle, as the limit, (beyond which no contrivance could carry it,) then the polisher must undergo an alteration. Its breadth should be diminished; the space, at the centre, left uncoated with pitch, should be greatly contracted; and, in the case of the little mirror, which has no perforation in it, entirely filled up; save that a small hole, through the polisher, tapering, from the back of it, upwards, to its surface, should be left, for the pitch to sink into, when it becomes closed, and too much compressed, at the centre; and the furrows, in the pitch, gradually deepened

* Here it may be proper to observe, that, as the curvature is constantly diminishing by the mere continuance of the operation, so the process is not to be pursued any longer, after the polish, and the desired figure, are found to be perfected. And the metal must always be brought to a very fine face, and a correct spherical figure, on the hones, or on a leaden tool, bedded with the finest washed emery, before the process of polishing commences; because if all scratches, from the grinding, be not previously obliterated, the polishing must, in order to efface them, be continued so long, as to diminish the curvature of the mirror beyond what is requisite; especially, if the area of the polisher be not of an oval, but of a round shape; which latter has a greater tendency, than the former, to diminish the curvature of the mirror and to render it hyperbolical. And the correction of this, afterward, might require a troublesome alteration of the polisher, or even make it necessary to put the metal again upon the hones; and yet, in the Gregorian telescope, the hyperbolic figure is the proper one, for either of the mirrors, if that of the other speculum be spherical.

toward

toward the edges. I believe, that (for the reason before given) the uncoated space, at the center, ought always to be as much smaller, on every side, than the perforation in the mirror, as the greatest range of the strokes, in polishing, advances the centre of the mirror beyond that of the polisher, having the same shape as the polisher itself; and that it ought to be smallest, or no other than as just mentioned, when there is no hole in the mirror.

Experience confirms this method.

I have, in this method, with certainty of success, as verified by examination of the progressive change of curvature in the mirrors, from a greater degree to a less, and *vice versâ*, effected the desired configuration of them: which serves to confirm me in the belief, that the circumstances, above proposed, are those which are really operative, in communicating the diversity of figure to telescopic mirrors; and that neither the direction of the strokes in polishing, the size or form of the polisher, consistence of the pitch made use of, or other accidents, are of any farther moment in the process, than as they serve to modify the resistance of the pitch, in the several parts of the surface of the polisher. Whether, by attention to the principles here laid down, it would be possible to produce an hyperbolic form, in the convex mirror of the *Cassegrain* telescope, I have been prevented from endeavouring to ascertain by experiment, from those casualties, affecting my situation in life, which I have already intimated. But it should seem, that it would, to a certain degree, be practicable, from the means I have suggested, of producing a progressive, specific alteration in the figure of the polisher; if I have judged rightly of the existence and cause of that alteration.

Proposed improvement of refracting telescopes.

And if it should be found possible to give, to a convex speculum, an hyperbolic curvature, the same could be done to the convex object glasses of dioptrical telescopes: which is a property still wanting to them; a want which makes them inferior to reflectors, even when they have been rendered achromatic, if the aberrations from their spherical figure remain, after those from refrangibility are removed: which aberrations, taken laterally, as they always ought to be, are as the cubes of the apertures. So that, if the lineal aperture be doubled, and the light admitted, which is as the square of the aperture, quadrupled, in order to increase the magnifying

magnifying power four times, (or to double the lineal amplification,) preserving an equal brightness in the image; the telescope must be made four times longer, that it may remain equally distinct *; an inconvenience, from which it must be very desirable to exempt this telescope, by correcting the figure, and, with it, the aberrations of its object glasses.

Observation of Huygens that very small pencils produce indistinct vision.

It is not the object of this essay, to investigate any particulars, in the construction of the telescope; (which would open a large field of inquiry;) but to try to assist mechanical contrivance in its fabrication. To this end, I think it fit to add a remark, on the property of the pencils of rays, respecting the latitude of each of them, where they fall on the pupil of the eye; first discovered by the great Mr. Huygens; as I suspect that the inconvenience he mentions, as resulting from a certain breadth of the pencil, may casually exceed the limit stated by him, and may admit of a practical remedy.

The author thinks that the indistinctness arises from faults in the object-glass, and not in the eye itself;

He observes, that if the latitude or breadth of the pencils, at the pupil of the eye, be very small, (so as not to exceed, if I remember right, the sixtieth part of an inch,) the vision, by the telescope or microscope, will be indistinct; so that, unless the pencils be of greater breadth than this space, at the place of the eye, the instrument will be defective: and he attributes this to something in the natural conformation, or in the humours of the eye; which, consequently, will admit of no remedy. On this may I presume

* A conception of this may be, perhaps, most familiarly acquired, by considering, that if, of two object lenses, the aperture, and also the focus of one, be twice as great as those of the other, the angles of incidence, and refraction, of the extreme rays, which come from a very remote object, and form the cones or pencils made by both, will be equal; and the pencils themselves, and their aberrations, will be similar figures; all the lineal measures of that of the larger lens, being double those of the other. In order, therefore, to reduce the lateral error or diameter of the circle of aberrations of the former, to an equality with that of the latter, while the aperture, which is the base of the pencil, remains double; this pencil must be made twice more narrow or acute than before. And, to effect this, its length or focus (which, at first, was twice as great as that of the other pencil) must be doubled; so that now it must be four times as long as the smaller pencil; i. e. the lengths should be as the squares of the apertures.

to

to observe, that the latitude of the pencil, as it enters the eye, is the same as that with which it falls on the last eye-glass; and, that the effect of it, which Mr. Huygens attributes to the eye, may, therefore, as naturally, be attributed to the eye-glass, as to the eye*; especially when an anatomical dissection of it will demonstrate, that the perfect transparency of its humours, and exquisite polish of its cornea, or outer coat, (not to talk of its achromatic property,) far exceed, in this optic instrument of the Deity, any thing that can be manufactured by man. In fact, the polish given to the eye-glass, and the transparency of the glass itself, is always imperfect; and many points in its surface, which ought to serve for the regular transmission of light, will be obstructed by its roughness and opacity; so that, if the pencils occupy but a very little space on the lens, no points of fair transmission may there remain; and the few rays, that pass through, may be so distorted, by irregular refraction, and inflection, in the glass, and in the eye-hole, that the vision must be indistinct. And this was the more likely to happen, in Huygens's time; because, neither the fine polishing powder, of colcothar of vitriol, was then in use; (and Mr. Huygens used nothing but tripoli;) nor was the method of polishing, by the help of pitch, divulged by Sir Isaac Newton. If this conjecture be right, the remedy is, to use both these helps, in communicating an exquisite polish to the eye-glasses; especially the smaller one, where the breadth of the pencils is reduced, in the same proportion as its radius, or as the increase of its magnifying power; and, also, to avoid using flint glass for this purpose; as it is found to be the least transparent of any, as well as most dispersive.

and therefore proposes that these should be improved as to their materials and polish.

I may here also observe, that, as the transparency and polish of glass must ever be, to a certain degree, imperfect; so the projected improvement of Mr. Ramsden, to avoid the dispersion of the rays, by throwing the image just before the first eye-glass, is unlikely to answer: because, in this case, each of the pencils would occupy little more than

Hence Ramsden's eye-piece is objected to.

* When a white ground is viewed through a telescope which gives a very slender pencil of rays, the want of transparency in the parts of the eye is seen by spots and other figures, which move along with that organ while the telescope is kept motionless.—N.

a point,

a point, on the surface of the lens; and, if that point should be opaque, or unpolished, or covered with dust, no rays of the whole pencil could be transmitted through it: which would, probably, happen to too many of the pencils, and affect the image, which is composed of these pencils; whose latitude, therefore, ought to be greater than the limiting measure stated by Mr. Huygens. And, to effect this, with as high a charge as the instrument will bear, a part of the amplification should be thrown away, on the first eye-glass; and diminished, by shortening its focus; that the pencils being, by it, rendered more obtuse, may fall, with greater divergence, and latitude, on the second eye-glass, which may thus be made shorter, and on the eye. For it is not to be supposed, that the image, formed in a telescope, can be viewed, by a small lens, with as much advantage as an object may. The rays, from each point of the latter, fall upon the whole surface of the lens; and, therefore, a sufficient number of them, to fill the pupil of the eye, must be transmitted; whereas, the rays, from each point of the image, occupy only a small speck on the lens; in no case larger, in effect, than the pupil of the eye; and must, when so contracted, be the more obstructed, by any imperfections in the glass.

The common colcothar objected to.

As, therefore, the fineness of the powder, employed in giving the highest lustre and polish to the specula, and to the eye-glasses, is of great importance, in that process: and, as I found, that the crocus, or colcothar of green vitriol, (now used, as the fittest for that end,) could seldom be procured, so free from grittiness, as to be capable of levigation, to a sufficient degree of fineness; insomuch, that I was obliged to attempt to make it myself; it may be useful to state, that, by the following easy method, I succeeded in producing some of excellent quality.

The author makes his colcothar by slowly roasting the copperas, or green sulphate of iron.

Considering that the vitriol, distilled in close vessels, might probably contract this grittiness, in its calx, from an union of some of its component parts, or principles, with the water contained in the vitriol, (which is the metallic salt of iron,) and that this might prevent its perfect calcination; I thought it best to perform the distillatton in the open air, and to begin with exhaling the water. Accordingly, I commenced with slowly roasting the vitriol or copperas,

peras, broken into grains as small as shot, by holding it over the fire, on a fire-shovel, till the moisture in it appeared to be dried away. I then put it into a crucible, and kept it uncovered, in a clear fire, till it had been some time red hot*; by which, the spirit or oil of the vitriol was distilled from it, and the calx or colcothar remained, of a brownish red colour, and of a perfectly equal texture, entirely free from the hard or gritty particles; and it was easily levigated, when moistened with water, on a piece of looking-glass-plate, by a piece of the like glass having a handle of brass cemented to it. This furnished a very fine and impalpable powder, capable of communicating to the specula, or lenses, the most exquisite polish and lustre †.

And levigating the oxide with water.

To apply with precision, and afford a fair trial of the method of polishing I have recommended, it is necessary farther to consider, that the advantages, resulting from correctness of figure, in the mirrors, may be frustrated, by an undue position of them, or of the lenses, in the instrument, or by a defect of form in the lenses, whose edges may happen to be thicker on one side than on the other; i. e. they may not be complete, or equally curtate segments of spheres; and, consequently, that a proper trial and estimate cannot be made, of the figure of the mirrors, unless these and the eye-glasses be right in these respects; especially in the Gregorian telescope, whose adjustment is a matter of more nicety and difficulty, than that of the Newtonian. And since, in the former, the surfaces of the mirrors and lenses ought all to have one and the same axis, viz. that of the instrument, in which are to be all their foci; it is necessary this should be cautiously ascertained; because contrary deviations of them, in this respect, might apparently compensate one another, and escape detection, though they would really be attended with the aberrations of enlarged apertures.

Good speculums may form a bad telescope if either the specula or the lenses be badly centered, or not placed on the same axis.

* I suppose that the fire ought not to be too high, or too long continued in this process, lest it should convert the calx of the iron into glassy scoria. Experiments will determine the due regulation of the heat, so as to ensure success to the operation in every instance. The heat ought to be so great, as to give the colcothar, not a brown, but a red colour.

† The same powder, spread on leather, would give the smoothest edge to razors and lancets, &c.

Directions for placing the mirrors.

The centre of the great mirror ought to be in the common axis of the instrument; and the position of it in its cell, in the tube, may be known thus. Let the little mirror be taken out of the tube; and let the round, central diaphragm, before-mentioned, (which ought to be made of a flat piece of tinned plate, or a brass plate, made clean and bright enough, to reflect the light strongly, but not polished,) be fastened across the mouth of the tube, exactly in the middle of it; and let a round hole be made through the plate, the centre of which shall be in the axis of the tube, and its diameter so large, as that the whole disc of the sun may be viewed through it, at the eye-hole of the telescope, when the eye-glasses are taken out. Then, directing the instrument to the sun, or the full moon, when very bright, so as that its whole disc shall be seen through the hole in the diaphragm; (using a lightly tinged screen-glass, to look at the sun;) if the light, reflected from the great mirror to the diaphragm, occupies on it, a circular area, concentrical with the hole made at its centre, the mirror is rightly placed, and its focus is in the axis of the tube. But, if the edge of the illuminated circle approaches nearer to the hole in the plate, on any side, the same side of the mirror inclines toward the axis of the tube, its cell not being exactly vertical to it; or otherwise, the centre of the mirror is not in that axis, as it ought to have been. If the outer one, of the three aforesaid diaphragms, be, at the same time, applied to the mouth of the tube, so as to expose only the middle zone of the great mirror to the light; the circle of light, reflected by it, to the central diaphragm, will appear better defined on it.

The speculums may be very truly set by the solar rays,

But the adjustment of both mirrors and lenses may, at the same time, be proved, by the following most easy and certain method, if exactly pursued:

Having provided, that the little mirror shall be so supported, that its centre may always move in the axis of the great tube; and proved that it is so, as Mr. Edwards prescribes, by taking off the mirror, and seeing, through the eye-tube of the telescope, (without the lenses,) that the hole, in the middle of the little round plate, to which that mirror is screwed, concentrical with the plate, corresponds with the intersection of two cross hairs, tied diametrically

across

across the mouth of the tube: then let the little mirror be replaced, and the eye-tube taken out, and let the telescope be directed to the sun's centre; which may be done, by the help of the little dioptrical telescope, called *a finder*, affixed to the instrument, if it be furnished with such; or otherwise, may be effected, with sufficient exactness for this purpose, by pointing the telescope to the sun, so as that the shadow of the little mirror may coincide with the hole, in the great mirror; which will be, when the great tubo is so placed, as to project its shadow of a circular form. In these circumstances, let the light, reflected from the little mirror, through the round perforation in the great one, be received upon a plane, placed at some distance behind the latter, at right angles to the axis of the tube. The light will occupy a circular area on the plane; and, if the centre of tho light be coincident with that of the shadow of the little mirror, this mirror is not only parallel to the great one, but both are duly adjusted, at right angles, to the axis of the tube; which, also, then corresponds with their axes. But, because the little mirror and its shadow, and also the cone of light, reflected from this mirror, aro of greater breadth than the perforation in the great one; the boundaries of the reflected light, and those of the shadow, cannot be seen wholly on the plane, through the hole in the great mirror, in any one position of the telescope. Let, therefore, the axis of the telescope be a little diverted from the centre of the sun, till the shadow of the edge of the little mirror falls within the hole in the great one: by which, some direct light will pass through, next the shadow, and appear on the plane, in form of a crescent: and, at the same time, the circle of the reflected light of the sun will have moved across the shadow; till, by a certain degree of obliquity, in the direction of the telescope, the edge of the circle of the reflected light will be in contact, externally, with the crescent of the direct light. And, if the crescent be always of the same breadth, when this contact takes place, on every side, by a diverting of the telescope, from the centre of the sun, successively, in every direction; then both the mirrors are parallel, and at right angles, to the axis of the instrument. But, if the crescent be broader, in any certain position of the tube, when the circle of reflected light just touches its

 edge;

edge; then the side of the little mirror, corresponding with that of the illuminated circle, where it is in contact with the crescent, makes too great an angle, with the axis of the instrument; and it must be reduced to a right angle, by screwing forward the adjusting screw of the little mirror, in that part, having previously withdrawn the opposite screws. When the mirrors are thus found to be rightly placed, and the eye-tube and lenses are restored to their places; if the whole image, of the great mirror, be not visible in the eye-hole, when this is in the common axis of the instrument, then the lenses are defective; either, some of them, or some of their surfaces, or the tube they are fixed in, being inclined to the common axis; and, by this means, distorting the cone of rays, from it. Which irregularities must be rectified, before a true estimate can be formed, of the correctness of the mirror's curvature.

— and the eye lenses afterwards set.

Position of the eye-hole.

The distance from the smaller lens, at which is the point of decussation of all the pencils of light, and the place where the eye-hole ought to be, may also be most easily and most accurately found, by directing the telescope to the sun, having taken off that part of the eye-tube behind the lenses; and letting the light fall on a plane, moveable back and forward, behind the second eye-glass, and kept at right angles, to the axis of the specula and lenses; the place, at which the image of the great mirror, with the shadow of the little mirror described on it, is seen most distinctly formed on the plane, ought to be the place of the eye-hole.

IV.

On the Absorption of Electric Light by different Bodies, and some of their Habitudes with respect to Electricity. In a Letter from Mr. Wm. Skrimshire, Jun. *Communicated by Mr.* Cuthbertson.

Wisbech, Jan. 1?, 1807.

Dear Sir,

As you thought my former account of some electrical experiments on the phosphorescency of calcareous substances worth sending to Mr. Nicholson's respectable and valuable Journal, I now trouble you with a second letter, containing the results of other experiments, made with the same view, upon the different species of the remaining earths that were within my reach. But I do not intend to stop here.—And as I perceive the chief value of my inquiries will rest upon their being a dépôt of numerous facts, whereon other philosophers may more successfully build than my abilities will enable me to do, it becomes necessary for me in this, and any future communication upon the same subject, to speak more in detail than I at first intended. If in this state, you should still think them worth public attention, I shall be glad to see them inserted in the Philosophical Journal.

Phosphorescence of bodies by electricity.

Barytic Genus.

Carbonate of barytes afforded no spark, but was very luminous when the shock was passed above it, though the light was of short continuance.

Barytic compounds.

Sulphate of barytes.—The several specimens of this substance, which were subjected to experiment, were all luminous by the electric shock, but were not so brilliant as the carbonates; they also differed from them in giving very good sparks when placed upon the prime conductor; except some specimens, consisting of small crystals, which acting as so many points, gave out the electric fluid to the atmosphere, and would not allow a spark to be taken from them. It is curious, and worthy observation, that in these two barytic species the facts turn out exactly the reverse of what takes place

place in the calcareous genus, in which the carbonates give sparks, though they are slightly luminous, compared to the sulphates, which are brilliantly phosphoric, but afford no spark; whereas in the barytic genus the carbonates are beautifully luminous, but give no sparks, while the sulphates afford good sparks, but are slightly phosphorescent.

Sulphuret of barytes.

Sulphuret of barytes was but slightly luminous by the electric explosion; in which it essentially differs from the sulphuret of lime *, which is the most brilliant phosphorus, both by the electric and solar light, that I have yet seen.

Muriatic Genus.

Muriatic genus.

Magnesia, pure, and carbonated, were both luminous by the electric explosion; the light, however, continued but for a short time.

Sulphate of Magnesia is very luminous through its whole substance.

Sulphuret of Magnesia is luminous, but not more so than the carbonate.

Turkey tobacco-pipes.—The bowls of these articles afforded tolerable sparks, but were scarcely luminous, except in the track of the electric fluid, when the points of the discharging rods rested *upon* the surface.

Chlorite gave sparks, which, upon its surface, branched off in minute, different-coloured points, something similar to, though not so brilliant as, the spark taken from any lacquered substance, such as gilt leather, or lacquered wooden ornaments. The explosion rendered it luminous.

Steatites, Talc, and fibrous Amianthus, gave sparks, and were slightly luminous by the explosion.

Asbestus.—A thin polished slab of this stone gave sparks similar to Chlorite, but the ramifications upon its surface were more numerous, and more variegated.

Mica gives sparks.

Mica affords sparks, but I could not observe it luminous by the explosion, When held in the hand it allows the sparks to run along its surface, to strike the finger at a considerable

* Canton's phosphorus is so readily illuminated by electricity, that a large lump, newly made, partaking of the exact shape of the crucible, and having never been exposed to light, being placed upon the prime conductor, was beautifully bespangled with brilliant spots, merely by taking the spark from various parts of its surface.

siderable distance, and to which it gives the sensation of a shock, rather than that of a spark. When the shock is passed along its surface, the fluid leaves an indelible mark, similar to its effect upon glass.

Micaceous Schistus gave a spark, which was ramified upon the surface of the stone, and somewhat more coloured than in the Chlorite. It was scarcely phosphorescent, except in the track of the electric fluid.

Argillaceous Genus.

Alum affords a purple spark, which is rather ramified upon its surface. It is luminous through its whole substance, when the explosion is made *above* its surface; but it is shattered to pieces when the shock is passed through it. Argillaceous genus.

Pipe-clay gave a spark, and was luminous by the explosion; but when it was formed into tobacco-pipes, and baked, it was scarcely if at all luminous, though it continued to afford sparks.

A greenish, foliated clay, found near the surface of the ground at Derby, gave a tolerably good spark, and was very phosphoric by the explosion taken *above* it. The luminous track left by the electric fluid, when the points of the dischargers rested *upon* the surface, continued several minutes.

A blueish clay *, dug at Wisbech, and provincially termed Gault, or Golt, affords a spark, and becomes luminous by the explosion.

Slate clay, or Shale, affords a spark, and is luminous; but from trials made with different specimens, it appears to lose its power of absorbing light in proportion as it becomes bituminous †.

Slates.—All the slates afforded sparks, and absorbed

* This clay, which is frequently dug in the Fens, near Wisbech, contains sulphur, and if, when fresh dug, it be held before a fire, it gives out a strong suffocating odour of sulphurous acid.

† At the time of writing the above observation on bituminous shale I had not the most distant recollection of a paper on the light of natural phosphori, by M. Carradori, translated in an early number of the Philosophical Journal, where it is mentioned, that luminous rotten wood, and other such like substances, "become phosphorescent in proportion as they have lost their inflammable principle, and that the property of absorbing, and retaining the light, depends on that circumstance."—Nicholson's Journal, 4to series, Vol. II p. 135.

electric

Argillaceous genus.

electric light from the explosion. But a slate used in this neighbourhood, brought from Colly Weston, in Northamptonshire, and which effervesces with acids, is by far the most beautiful. When the shock is taken *above* the centre of a piece some inches square, not only the part immediately below the rods is luminous, but the surface of the slate appears bespangled with very minute brilliant points to some distance from its centre; and when the points of the dischargers rest upon the surface of the slate, these minute spangles are detached, and scattered about the table in a luminous state. The track of light between the rods continues phosphorescent several minutes.

Hone stone, of a dirty greyish colour, gave a good spark, and was phosphoric by the explosion.

Fuller's Earth gave a good bright spark, but was very slightly luminous, except in the track the fluid left in its passage from one rod to the other.

Reddle gave no spark, but a purple stream, attended with a very sharp hissing sound. It was rather more luminous than Fuller's Earth.

Armenian Bole affords a spark, which is ramified upon its surface. It is not luminous by the electric explosion; even when the points of the dischargers rest upon it, no track of light is visible; but several minute fragments are shivered from its surface.

Terra Sigillitica of the shops gives a spark, and is phosphoric after the explosion.

Basalt gives sparks which are radiated upon its surface, but not ramified as in Chlorite and micaceous Schist. It was phosphoric only in the track formed by resting the dischargers upon it.

Bricks of various kinds afforded small purple sparks, of a bright red colour on the surface. They were very slightly luminous by the explosion; but afforded a bright track of light between the points of the dischargers when they rested upon them.

Tiles of different kinds afforded similar results to the bricks, except that most of the tiles were rather more luminous than the bricks, when the electric explosion was made above them, especially a yellow tile, with a redish tinge in the fracture, which, from its greater phosphorescence,

cence, and its slightly effervescing with acids, I suspect to contain more calcareous earth in its composition than the others.

Queen's ware, glazed, gives a good spark, which is flame coloured, and radiated on its surface; but it is not phosphoric. When fractured, the unglazed surface affords a purple spark, and is luminous by the shock.

All the different kinds of Pottery-ware which I tried gave the same results, except slight varieties in the colour of the sparks: and a common dirty white ware, which was luminous on its glazed surface when the shock was passed above it. From what I have hitherto observed, I am induced to believe that all glazing, in which a metallic oxide is used, is not phosphoric, but gives a good spark.

All unglazed Pottery is luminous by the explosion, and gives a vivid track of light when the dischargers rest upon its surface.

Siliceous Genus.

Rock Crystals were all phosphoric by the explosion; and some of them that had two or three particles of ore upon their surfaces, were transparent by the spark when it passed from the ore to the knob of the discharging rod, otherwise the crystals gave no sparks, but merely a hissing stream. A rhombic crystal was rendered luminous through its whole substance by the explosion, retaining its light several minutes. At the instant after the explosion it emitted a red light, but which very soon became white. Siliceous genus.

Siliceous sand, washed and dried, was not luminous, except where the points of the dischargers were in contact with it.

Quartz is phosphoric, and shines with a uniform dull white light. It gives a purple stream instead of a spark. After the explosion it affords the same odour as when two pieces are rubbed together.

Flints afford small purple sparks, both from the external coat and the surface of the fracture. The explosion does not render them so luminous as Quartz, but the external coat is more phosphorescent than the fracture, especially in the track of the discharge.

 Lapis

Siliceous genus.

Lapis Lazuli affords a very good spark, and is luminous by the shock.

Egyptian Pebbles, Scotch Pebbles, Felspars, Agates, Calcedonies, Carnelians, and Jaspers, gave hissing purple sparks, and were luminous by the explosion. Several of these substances give out the same odour as when two pieces are rubbed against each other.

Porphyries and Granites gave a hissing purple spark, and were luminous by the shock, which, when passed upon the surface, produced a very bright track of light, which in some specimens, especially in a piece of whitish Granite, continued luminous for several minutes.

Pudding Stones gave a similar hissing spark; and the oval pebbles being more luminous than the siliceous sand, in which they are imbedded, were readily distinguished in the dark when the shock was passed above the surface of the stone.

Mochoas gave very good sparks from the arborescent parts, but only a hissing stream from the stone itself, which is slightly luminous by the shock, but affords a bright track of light between the ends of the discharging rods.

The Yorkshire Stone, which is used for flat pavement, gives a purple spark, and seems to become luminous by the electric explosion, in proportion as it partakes of a calcareous nature, for those specimens which are verging toward micaceous schist, (and in which I have found lamina of sulphur nearly the tenth of an inch thick,) are scarcely at all luminous.

Pumice Stone on some parts of its surface gave only a hissing stream, but on others very good sparks, which appeared to penetrate through its substance, as if it contained some metallic particles within it. The shock rendered it slightly luminous, but it afforded a very bright track of light along its surface, between the ends of the dischargers.

The semivitrified ashes of a Haystack, which was consumed by spontaneous combustion, gave only a hissing stream, and was slightly luminous by the electric explosion; but when the shock was passed upon its surface it afforded a bright track of light.

Various kinds of Glass are not luminous, neither do they give

give a spark. But the dark green glass of which wine bottles are made, when by exposure to air and moisture, or under other circumstances which may enable it to reflect the prismatic colours with brilliancy, is capable of giving a spark, and emitting light, after the electric explosion has been made above its surface.

Strontian Genus.

Native Carbonate of Strontites, instead of a spark, gave only a hissing purple stream, but was very luminous by the explosion. Strontian genus.

I remain,
Your's, &c.
W. SKRIMSHIRE, Jun.

V.

Observations upon the Marine Barometer, made during the Examination of the Coasts of New Holland and New South Wales, in the Years 1801, 1802, *and* 1803. *By* MATTHEW FLINDERS, *Esq. Commander of his Majesty's Ship Investigator. In a Letter to the Right Hon. Sir* JOSEPH BANKS, *Bart. K.B. P.R.S. &c. &c. &c. From Philosophical Transactions for* 1806.

Isle of France, Aug. 19, 1805.

A FORE-KNOWLEDGE of the wind and weather is an object so very interesting to all classes of men, and the changes in the mercurial barometer affording the means which appear most conducive to it, a system that should with certainty explain the connection between the variations of the mercury and those in the atmosphere under all circumstances, becomes greatly desirable; to seamen, more especially, whose safety and success depend so much upon being duly prepared for changes of wind, and the approaching storm, it would be an acquisition of the first importance: in a more extended view, I may say, that the patriot and the philanthropist must join with the philosopher and the mariner in desiring its completion. So long and widely-extended a course of observation, however, seems requisite to form even

Great advantages of a fore-knowledge of the weather. The barometer indicates it.

even a basis for it, that a complete system is rather the object of anxious hope than of reasonable expectation. Much has been done toward it, but so much appears to remain, that any addition to the common stock, however small, or though devoid of philosophical accuracy, I have thought would be received by the learned with candour. With this prepossession, I venture to submit to them some observations upon the movement and state of the mercury upon the coasts of New Holland and New South Wales, the Terra Australis, or Australia, of the earlier charts.

The land and sea winds are more particularly indicated, and also their strength.

The principal circumstance that has led me to think these observations worth some attention, is the coincidence that took place between the rising and falling of the mercury, and the setting in of winds that blew from the sea and from off the land, to which there seemed to be at least as much reference, as to the strength of the wind or state of the atmosphere; a circumstance that I do not know to have been before noticed. The immediate relation of the most material of these facts, it is probable, will be more acceptable than any prefatory hypothesis of mine; and to it, therefore, I proceed; only premising, that a reference to the chart of Australia will be necessary to the proper understanding of some of the examples.

My examination of the shores of this extensive country began at Cape Leuwen, and continued eastward along the south coast. In King George's Sound, December 20, 1801, after a gale from WSW, the mercury had risen from 29,42 to 29,84, and was nearly stationary for two days, the wind being then moderate at NW, with cloudy weather. On the 22d, the wind shifted to SW, blew fresh, and heavy showers of rain occasionally fell; but the mercury rose to 30,02, and remained at that height for thirty hours; and on the weather clearing up, and the wind becoming moderate in *the same quarter*, it rose to 30,28.

Fresh breezes from E and SE caused a rise in the barometer in King George's Sound, once to 30,20, and a second time to 30,18, although the weather at these times was hazy: but with light winds from the same direction, which were probably local sea breezes only, the mercury stood about 29,95 in that neighbourhood.

2d Example. Jan. 12, 1802, in D'Entrecasteaux's Archipelago,

pelago, the mercury rose to 30,23, previously to a fresh breeze setting in from the eastward. In the evening of the 13th it blew strong from ESE, with hazy weather, and a rapid fall of the mercury to 29,94 had then taken place; but instead of the wind increasing, or bad weather coming on, the wind died away suddenly, and a light breeze came off the land at midnight, with cloudy weather.

Observations and inferences to ascertain the correspondent changes of wind and weather to be expected after change in the marine barometer.

At the Cape of Good Hope, which is nearly in the same latitude, the mercury rises with the fresh gales that blow there from the SE in the summer season. The weather that accompanies these south-east winds, is nearly similar in both places; the atmosphere being without clouds, but containing a whitish haze, and the air usually so dry as sensibly to affect the skin, particularly of the lips.

3d. Jan. 22. Three degrees east of the Archipelago, the mercury fell with some rapidity down to 29,65 with the wind from ESE. It was eight o'clock at night, and we prepared for a gale from that quarter; but at ten, the wind suddenly shifted to WNW, coming very light off the land. On its veering gradually round to SSW, clear of the land, at noon, 23d, it freshened, and the weather became thick; yet the mercury had then risen to 29,84, and at eight in the evening to 29,95, though the wind then blew strong. It continued to rise to 30,16 as the wind shifted round to SE, and fine weather came on; but on the wind passing round to ENE and NNE, which was off the land, the mercury fell back to 29,73, though the weather was fine and the wind moderate. On a sudden shift of wind to the SW, a fresh breeze with hazy weather, it again began to ascend, and a similar routine of wind, producing nearly the same effects upon the barometer, again took place. The effect of sea winds in raising the mercury, in opposition to a strong breeze, and of land winds in depressing it, though they were light, was here exemplified in two remarkable instances.

4th. In the neighbourhood of the Isle of St. Francis of Nuyts, longitude 133½° east of Greenwich, we experienced a considerable change in the barometer. For nine days in January and February the wind continued to blow constantly, though moderately, from the eastward, mostly from the SE. It appeared like a regular trade-wind or monsoon, but so far partook of the nature of sea and land breezes, as commonly

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commonly to shift more to the southward in the day, and to blow more from east and NE in the night. The weather was very hazy during these nine days; so much so, that for six of them no observation of the sun's altitude, worthy of confidence, could be taken from the sea horizon, although the sun was sufficiently clear; and in the whole time, the mercury never once stood so high as 30 inches, but was frequently below 29,70. I considered this to be the more extraordinary, as settled winds from the eastward, and especially from SE, had before made it rise and stand high upon this coast, almost universally, even when there was a considerable degree of haze. The direction of the south coast, beyond the Isle of St. Francis, and even abreast of it, was at that time unknown to me; but I then suspected, from this change in the barometer, that we should find the shore trending to the southward, which proved to be the case. The easterly winds, then, whilst they came off the sea, caused the mercury to rise upon the south coast; but in this instance that they came from off the land, they produced a contrary effect; but it is to be observed, that the most hazy part of the time, and that during which the mercury stood lowest, was two days that the wind kept almost constantly on the north side of west, more directly off the land: its height was then between 29,65 and 29,60.

The haze did not immediately clear away on the wind shifting to the westward; notwithstanding which, and that the new wind rose to a strong breeze, and was accompanied with squalls of rain, the mercury began to ascend, and had reached 29,95 when the squalls of wind and rain were strongest; the direction of the wind being then from SSW. On its becoming moderate, between SSW and SSE, the mercury ascended to 30,14, and remained there as long as the wind was southwardly.

5th. Going up the largest of the two inlets on the south coast, in March, we were favoured with fine fresh breezes from SSW to SSE, sometimes with fine, sometimes with dull weather, the mercury rising gradually from 30,08 to 30,22. In twenty-four hours afterwards, it fell below 30 inches, and a light breeze came from the northward, off the land, with finer weather than before. The mercury continued to fall to 29,56, where it stopped; the wind having then ceased to blow

blow steadily from the northward, and become variable. In twenty-fonr hours more, the wind set in again to blow fresh from the southward, the mercury having then returned to 29,94, and it was presently up to 30,22 and 30,28. It kept nearly at this height for several days that the southwardly wind blew fresh, but on its becoming lighter, and less steady in its direction, the mercury descended; and in the calm which followed, it had fallen to 29,90. This example affords clear proof of a fresh wind from the sea making the mercury rise, whilst a light wind off the land, with finer weather, caused it to descend.

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6th. The calm was a prelude to a fresh gale; but the mercury began to rise at eight in the evening when it had just sprung up; by the next noon it was at 30,10 when the wind blew strongest, and in the evening at 30,22. This gale began as gales usually, if not always do upon this coast, in the north-west quarter, and shifted round to SW and SSW; but quicker than I have generally seen them: there was no rain with it, nor was the atmosphere either very hazy or cloudy*. The mercury continued to rise till it had reached 30,25, and then was stationary as long as the wind remained between south and west; but on its veering round to the eastward of south, a second rise took place, and for forty hours the mercury stood as high as 30,45, the wind being then between SE by S and east: the weather was very dull and hazy during the first half of these forty hours, but finer afterwards. As the winds between SE by S and east slanted off the main land, this example seems to be in opposition to the 4th, and leads me to think, that it might have been the very extraordinary kind of haze, and perhaps some peculiarity in the interior part of the land abreast of the Isle of St. Francis that in part occasioned the fall of the mercury with south-east winds; as much, perhaps, as the circumstance of the wind coming from off the shore.

After this rise in the mercury to 30,45, it fell gradnally; but, for thirteen days, kept above 30 inches, the winds being generally between SE and SW, but light and variable, and the weather mostly fine.

* I afterwards learned from Captain Baudin, that this gale was much heavier in Bass' Strait than we felt it at Kanguroo Island.

7th

Observations and inferences to ascertain the correspondent changes of wind and weather to be expected after change in the marine barometer.

7th. North-eastwardly winds, off the land, were the next that prevailed; they were light, and accompanied with cloudy weather and spitting rain. The mercury fell to 29,70, and remained there till the wind shifted to the west and southward, when it began to rise, and in two days was up to 30,42. At that time we were off the projection marked H. in the chart, in 139½° east longitude; the wind had then veered to the south-eastward along the shore, with a steady breeze, and the mercury remained nearly stationary so long as it lasted; but on the wind dying off, and flawing from one side to the other, it descended quickly to 30 inches. A breeze then sprung up at NW, which, within twenty-four hours, shifted suddenly to SW, and blew a gale which had near proved fatal to us. It was accompanied with rain and very thick weather, and lasted two days; by which time, the mercury had descended to 29,58.

8th. In Bass' Strait, for several days in the month of April, the mercury stood above 30,40 with the wind from the south and eastward, sometimes blowing fresh: the weather generally fine. It then fell half an inch in eight hours, and a wind set in soon after from the north-westward which continued four days, blowing moderately, with cloudy weather, and sometimes a shower of rain; the mercury remaining stationary between 29,83 and 29,89. On this second wind dying away, a strong breeze sprung up which fixed at WSW with squally weather; but for three days no alteration took place in the barometer, until the wind shifted to NW and north, and the mercury then descended to 29,52, though the weather was finer, and wind more moderate than before.

9th. Passing along the south coast of Australia the second time, we experienced light winds from the sea for forty hours in D'Entrecasteaux's Archipelago, in the month of May: they were variable between WSW and SSE with dull cloudy weather, and the mercury stood very high, being up to 30,50 most of the time. The wind then came round to N by E and NNW; previously to which, the mercury began to descend, and it kept falling for two days till it reached 30,19, though the weather was not so cloudy as before, and the wind was equally light. On the wind veering to west and WSW the mercury rose to 30,25; but it

it now came on to blow fresh, with squally thick weather, yet the mercury continued nearly stationary for twenty-four hours, appearing to be kept up in consequence of the wind having shifted round to SSW, more directly from off the sea. On its increasing to a gale, however, there was a pretty rapid descent in the barometer to 29,96; but the ascent again was equally rapid, and to a greater height, on the wind becoming moderate. In a short calm that succeeded, the mercury stood at 30,42, but on the wind setting in from the north, which was from off the land, it fell to 30,25, and remained there two days: we had then reached Bass' Strait.

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From these examples upon the south coast, it appears, generally, that a change of wind from the northern, to any point in the southern half of the compass, caused the mercury to rise, and a contrary change to fall; and that the mercury stood considerably higher when the wind was from the south side of east and west, than, in similar weather, it did when the wind came from the north side; but, until it is known what are the winds that occasioned the mercury to ascend, and what to descend, upon the other coasts of Australia, it will probably be not agreed, whether it rose in consequence of the south winds bringing in a more dense air from the polar regions, and fell on its being displaced by that which came from the Tropic;—or whether the rise and higher standard of the mercury was wholly, or in part, occasioned by the first being sea winds, and the descent because those from the northward came from off the land.

The height, at which the mercury generally stood upon the south coast, seems to deserve some attention. It was very seldom down to 29,40, and only once to 29,42. Of one hundred and sixty days, from the beginning of December to May, it was nearly one-third of the time above 30 inches; and the second time of passing along the coast, from the 15th of May to the 1st of June, it only descended to 29,96, and that for a few hours only, its average standard for these sixteen days being 30,25. Upon the eastern half of the coast, beyond Cape Catastrophé, in March, April, and May, the mercury stood higher than it did on the western half in December, January, and February: the average standard of the first was 30,09, but that of the

 latter

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latter only 29,94. At the Cape of Good Hope, the mean height in the barometer, during eighteen days in October and November, was 30,07.

The marine barometer on board the Investigator, supplied to the astronomer by the Board of Longitude, was made by Nairne and Blunt, and had, I believe, been employed in one or more of the voyages of Captain Cook, and perhaps in that of Captain Vancouver. I suspect, that it was not suspended so exactly in the proper place, as the latter instruments of these makers probably are; on which account, the motion of the ship caused the mercury to stand too high; and perhaps one or two-tenths of an inch might be deducted with advantage from the heights taken at sea, but I think not when the ship was lying steadily at anchor in the harbour. The barometer stood in my cabin, and the height of the mercury was taken at day-break, at noon, and at eight in the evening, by the officer of the watch; as was also that of the thermometer.

The general effects produced upon the barometer by the sea and land winds, on the east coast of Australia, will be learned from the following abridgment of our meteorological journal:

1*st*. In the run from Cape Howe, in 37½° south latitude, to Port Jackson, in 34°, once in the month of May, and once in June, I found that the mercury descended with light winds from north, NW, west, and WSW; whilst during fresh breezes from south and SW it ascended, and stood considerably above 30 inches; with the wind at NE and NNE it also kept above 30 inches, but not so high, nor did it rise so fast, as when the wind was from SSW. From between south and east, the winds did not blow during these times. This example does not differ so much from those on the south coast as to be decisive of any thing.

2*d*. The observations made during a stay of ten weeks at Port Jackson, in May, June, and July, 1802, are more in point than almost any other. Strong eastwardly winds were very prevalent at that time, and were almost always accompanied with rain and squalls; yet this weather was foretold and accompanied by a rise in the barometer, and the general height of the mercury during their continuance was 30,20: higher if the wind was on the south side of ESE, and lower

lower if on the north side of east. The winds from south and SSW, which blow along the shore, kept the mercury up to about 30,10, when they were attended with fine weather, as they generally were; but if the weather was squally, with rain, is stood about 29,95. During settled winds from between WNW and SW, with fine weather, the mercury generally stood very low, down at 29,60 †; and what is more extraordinary, when these winds were less settled, and the weather dull, with rain occasionally falling, the range of the mercury was usually between 29,80 and 30,10; nearly the same as when the wind was at SSW with similar weather; the reason of which may probably be, that at some distance to the southward these westwardly winds blew more from the south, and were turned out of their course, either by the mountains, or by meeting with a north-west wind farther to the northward.

Observations and inferences to ascertain the correspondent change of wind and weather to be expected after change in the marine barometer.

The winds from north and NW blew very seldom at this time: the mercury fell on their approach.

To the state of the mercury during our second stay at Port Jackson, in July, 1803, and part of June and August, it is not in my power to refer, as I have not been able to obtain that part of my jonrnal from General De Caën.

The effects of some winds upon the barometer in this 2d example, are considerably different to what they were upon the south coast. The wind at WSW or SW with fine weather, had always caused the mercury to rise and stand high, and those from the NE to fall; whereas here, the effects of those winds were almost directly the reverse. The winds from SSW, SE, and as far as east, made it rise on both coasts, with the exception of the 4th example on the south; and from between north and WNW the mercury fell in both cases and stood low.

3d. Steering along the east coast, from Port Jackson to the northward, in July, we had the winds usually be-

† My friend Colonel Paterson, F.R.S. commander of the troops at Port Jackson, in judging of the approaching weather by the rise and fall in his barometer in the winter season, told me, that he had adopted a rule directly the reverse of the common scale. When the mercury rose high, he was seldom disappointed in his expectation of rainy, bad weather; and when it fell unusually low, he expected a continuance of fine, clear weather, with westwardly winds.

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tween south and SW, and sometimes WSW, the mercury being nearly stationary at 30 inches; but meeting with a spurt of the south-east trade wind in latitude 24°, we found it rise to 30,30 for two days. A westwardly wind brought it back to 30 inches for a short time; but on the trade wind finally setting in, it fixed itself between 30,20 and 30,30, as long as the wind preserved its true direction.

4th. The month of September, 1802, and the greater part of August and October, we spent upon the east coast between the latitudes 23° and 17°. The south-east trade is the regular wind here, but we had many variations. Whilst the trade prevailed, the average standard of the mercury was 30,15, and the more southwardly it was, and the fresher it blew, the higher the quicksilver rose, though it never exceeded 30,30. When the trade wind was light, it was usual for a breeze to come off the land very early in the morning, and continue till eight or nine o'clock; but these temporary land winds did not produce any alteration in the mercury, which kept at these times about 30,10. When the trade wind veered round to ENE, or more northward, which was not seldom, the mercury ranged between 30 inches and 30,10; and when a breeze from north or N by W prevailed, which was the case for a considerable part of twenty days we remained in Broad Sound, its height was something, but not much less. These northwardly winds I take to have been the north-east wind in the offing; which had been partly turned, and in part drawn out of its direction, by the peculiar formation of this part of the east coast. There are but few instances of any steady westwardly wind prevailing; when such happened, they were generally from the north side of west; and at these times the range of mercury was between 29,95 and 30,05, which was the lowest I at any time saw it on this portion of the east coast.

The barometer was of great service to me in the investigation of this dangerous part of the east coast, where the ship was commonly surrounded with rocks, shoals, islands, or coral reefs. Near the main land, if the sea breeze was dying off at night, and the mercury descending, I made no scruple of anchoring near the shore; knowing that it would either be a calm, or a wind would come off the land; but if

if the mercury kept up, I stretched off, in the expectation that it would freshen up again in a few hours. Amongst the barrier reefs, when the wind was dying away, the barometer told me, almost certainly, from what quarter it would next spring up. If the mercury stood at 30,15, or near it and was rising, I expected the proper trade wind; and if higher, that it would be well from the southward, or would blow fresh; and if it was up to 30,30, both. The falling of the mecury to 30,10 was an indication of a breeze from the north-eastward; and its descent below 30 inches that it would spring up, or shift round to the westward. This regularity of connection between the barometer and the direction of the wind, is perhaps too great to be expected at a different time of the year; and it is probable, that we should not have found it continue so strictly, had our stay amongst the barrier reefs been much prolonged.

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5th. Leaving the east coast in the lat. 17° south, we steered off to the northward for Torres' Strait, in the latter part of October. As we advanced northward, I found the mercury stand gradually lower with the same kind of wind and weather. The difference was not material till we reached the latitude 13°, but afterwards, the south east wind which had before kept the mercury up to 30,15, then permitted it to fall to 29,90; and the winds from ENE and NNE to 29,85. Was this alteration owing to the want of density in the air brought in by the south-east winds, in this lower latitude?—to our increased distance from the land?—or was it, that the south-east wind was no longer obstructed by the coast, having a passage there through Torres' Strait?

The difference between the height of the mercury, during a north-east and a south-east wind, was much less here than before, although the weather was most unfavourable during the time of the north-east wind, and should have increased the difference in their effects. Was this owing to the general approximation to that equality which has been observed to take place in the barometer, in very low latitudes?—or that the north-east wind, still meeting with resistance from the coast, had one cause for keeping up its power, which the south-east wind had lost?

In a general summary of the winds on the east coast, those

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those that came from between south and east caused the mercury to rise and stand highest, as they had also done upon the south coast, with the exception of the 4th example. The winds from NE kept the mercury up above 30 inches on the east coast, and caused it to rise after all other winds except those from the south-eastward; but on the south coast, the mercury fell with them, and stood considerably below 30 inches; because, as it appears to me, they then came from off the land. During north-west winds, the mercury stood lower than at any other time upon both coasts; and on both they came from off the land.

Moderate winds from the south-westward, with fine weather, caused a descent of the mercury on the east coast; and during their continuance it was much lower than with winds from the north-eastward; but upon the south coast it rose with south-west winds, and stood much higher than when they came from the opposite quarter. For this change I cannot see any other reason, than that the wind, which blew from the sea upon one coast, came from off the land in the other.

Although the height of the mercury upon the south coast of Australia was, upon the average, considerably above the medium standard 29,50, it was still greater upon the east coast: I cannot fix it at less than 30,08 or 30,10, whereas upon the south coast, I should take it at 30 inches; both subject to the probable error of one or two-tenths of an inch in excess. This superiority seems attributal to the greater prevalence of sea winds upon the east coast, and particularly of those from SE, which, *when all other circumstances are equal*, I have observed to raise the mercury higher than any other on this side of the equator, analogous to the effect of north-east winds in the northern hemisphere; and perhaps also, the superiority may be in part owing to the east coast having a more regular chain of higher mountains running at the back of, and parallel to it, which presents a greater obstruction to the passage of the wind over the land, than it meets on the south coast.

(To be Continued.)

VI.

Letter from a Correspondent, on the Exhibition of the Invisible Girl.

To Mr. Nicholson.

Sir,

Bristol, Jan. 9, 1807.

The account of your correspondent X. of the manner in which the Invisible Girl amused the lounging public, exactly agrees with one which I sent to Mr. Walker, of Conduit Street, about two years ago, except that X. seems to have failed, as I did, in discovering the mode by which she saw the company; for one cannot be satisfied with being told, that "a small hole closed with glass is left through "the tunnel and side-wall of the room;" having carefully examined the room of that exhibited at Bristol, and ascertained that there could be no such aperture. Besides, we know that to see through any hole of a very small size the division must be nearly as thin as a sheet of paper, and a hole through a tunnel and side-wall must have been very long, indeed much too long to see people through. As my friend never answered that letter, I concluded that he either doubted of my account being the true one, or that he could not explain satisfactorily how the view of the company was acquired at Charles's exhibition; although he would not have been long at a loss to invent some expedient, had it been worth his while.

Confirmation of the former explanation of the Invisible Girl.

Question, How did she see the company?

In fact, thinking it might hurt the harmless exhibitors, or lessen the amusement of the public, I desired that account might not be published, unless necessary to prevent superstitious uses being made of the trick; and, after all, we lose by all these discoveries when made public, much innocent pleasure, as I well remember was the case when Mr. Thickness unveiled some such exhibitions. That which I saw at Bristol and Bath had a loose rail with eight legs; seven of which the operator always removed from their places to blunt suspicion, but the eighth I always found immovably fixed, and that was ever the leg toward the closet where the lady sat who directed us.

Account of an exhibition at Bristol.

His

His rail also was covered opposite the mouths of his trumpets with stained paper; but you could feel the vibration on the holes when any one answered, and peoples' hands had a little indented them by accidental pressure. As to a small camera, I do not think it was ever used here, or at all necessary for the lady, as a yard of tube with a trumpet mouth would have answered all the purposes: as, however, you have been at the pains of satisfying the general curiosity in so handsome a manner, excuse me if I request your correspondent to complete the instrument by disclosing what he actually knows of the mode of complete vision by direct or indirect reflection; being always,

Sir,

Your's,

G. C.

P.S. You have omitted *three letters* in the diagram of the perspective lines.

VII.

Description of a new permanent Compensation-Balance for a Time-keeper. By Mr. Wm. Hardy*.

Description of two compensation balances at present in use.

We have at present two compensation-balances; one sort consists of several slips of brass and steel soldered, or fluxed together, and disposed in form of two S S's on the balance, but this is almost now out of use. The other is a steel balance, having a rim of brass fluxed upon its outside, and cut open in two or three places, with sliding weights on the rim, to increase or diminish the effect of the balance. The nature of the balance (the only one now in use) is well known, as well as its defects, which it is unnecessary for me to state at this time, as I shall have a better opportunity of pointing them out at large, should I be ordered to attend the Society.

Objections.

Instead of this uncertain way of constructing a balance, which never continues long in the same state, but requires to

* Communicated to the Society of Arts, who voted a reward of thirty guineas.

to be adjusted every time the watch wants cleaning; I have rejected this mode altogether, and have contrived a method of applying the direct expansion of metals, which I find by experience to be constant and permanent in its effects.

My balance consists of a flat steel bar, which forms its diameter. Beneath this steel bar are two metallic rods, secured at one end by a stud, formed out of the steel bar, and the other end acting on the short end of a lever, formed out of the other end of the same steel bar, being made to spring at the place where the centre of the lever would fall; to this lever is fastened a small cylindrical stem of brass, upon which a small globe of brass slides or screws; there is also a screw passing through the stem, to serve to regulate to mean time. Another metallic bar, equal and similar, and furnished like the other, but reversed in position, is placed parallel to it.

The new balance described

Mode of acting.

When the whole balance is heated, the metallic rods will push forward the short ends of the levers, and which quantity will be just equal to the difference of the expansion of the two metals. Suppose the short ends of the two levers to be each equal to 1, and the long ends of the levers to be each equal to 20, then it is evident that the motion of each globe will be twenty times the excess of the steel bar and metallic rods nearer to the centre of the axis of the balance, than before the expansion took place; and, what is a very grand and necessary property in the motion of the two globes, they will always move directly to the axis of the balance; that is, their action will be constantly in a plane, passing through the axis of the globes and axis of the balance. To increase or diminish the expansion of the balance, will be only to slide or screw up or down the globes upon their stems, until the balance produces the desired effect.

Action of the new balance explained.

Additional Remarks on the Balance now in Use.

The rim of brass and steel of the common balance, however intimately connected when first fluxed together, are by every change of temperature endeavouring to break the connection, and do by little and little tear themselves asunder, at least in a partial degree, for the fracture is often

Manner in which the common expansion balance is supposed to fail.

often visible, so that the balance has nothing permanent in its nature. New adjustment is necessary much oftener than the instrument requires cleaning: but that adjustment is of no duration; for, as the pores are more torn than at first, the balance becomes worse and worse, and at last quite useless for what it is intended.

The new balance has no soldered or welded surface.

I make use of the direct expansion of metals; for the bars of my balance are independent of each other. They are connected only at the extremities, and the excess or difference of the expansion of the two metals is communicated to the short ends of the two spring levers. Its durability can therefore no more be doubted than that of the gridiron pendulum, where the direct expansion of metals produces the desired effect.

Its weights move nearly in a diameter.

The two globular weights were described in my last letter as moving constantly in the same plane, which passes through their centres and the axis of the balance; and I should have added that, as to sense, they also move in the same right-line which passes through the centres of the globes, and cuts the axis of the balance at right angles; for the versed sine of a very small arch, or the difference between the radius and co-sine, is in this case a quantity so small that it cannot be perceived; and however we increase or diminish the expansion of the balance, or whatever may be the degree of temperature, it still retains this admirable property, namely, that the two spherical weights move not only in the same plane in a strict mathematical sense, but also in the same right line in a physical one. This quality, united with the direct motion of the brass bars, renders the motion of the globes simple and uniform, and therefore the effect (depending on such simple and direct causes) is regular and certain.

A noxious vibration of the weights in the common balance.

The common balance, when in motion, causes the weights to fly off or recede from the axis of the balance, and this flying off will increase and diminish with the arch of vibration in the balance: for, as there is nothing to brace the rim at the extremity of which the weight is suspended; as the arch of vibration increases, the weight and rim are thrown outward as much as the centrifugal force of the weight exceeds that of the elasticity of the rim. And as the arc of vibration diminishes, and consequently the centrifugal

centrifugal force, the weight is thrown inward by the elasticity of the rim.

My weights or spheres are firmly braced in every degree of temperature, and consequently not influenced in the smallest degree by any change in their centrifugal forces; therefore, in every respect, this balance may be considered as permanent.

The great difficulty in constructing a balance, and in applying the direct expansion of metals, is to contrive it so as that it shall preserve its equilibrium in every degree of temperature, and also admit of having all its parts made perfectly equal and similar by mechanical means. Both these important problems I have solved, by the introduction and application of a different principle from any yet used in the construction of the balance of a timekeeper; and I am fully satisfied, from a variety of experiments which I have made, that by this total change of system, I have made a higher step towards the perfection of time-keepers, than has been effected by any other means that have come within my knowledge.

The new balance preserves its equilibrium.

*Letter to the Secretary, by the Editor**.

Dear Sir,

I take the liberty to express my opinion of the compensation-balance, which Mr. Hardy has submitted to the consideration of the Society of Arts. I think it a very excellent contrivance: the following are some of the reasons which, I presume, will entitle it to the approbation of that respectable Institution.

1st.—The invention of confining the flexure of the steel bar to a small part near the end is new, and no less remarkable for its ingenuity and simplicity, than for the steady effect it produces.

Advantages of Mr. Hardy's balance.

2d.—The whole combination is particularly firm; and as the workmanship depends upon faces which are either

* The useful and patriotic society to which this letter was addressed through their Secretary, is always ready to receive communications respecting the subjects proposed for their consideration.—N.

plain or turned in the lathe, it can very easily be manufactured without requiring uncommon skill in the workman.

3d.—As it has neither working surfaces of contact, nor joints nor levers, it will regularly obey the minute changes of temperature, and will not act by jerks or starts.

Explanation of the mode of action in bars of brass and steel fused or soldered together.

4th.—In the expansion-bar consisting of two metals, connected longitudinally by soldering or otherwise, the differences of length between them, when heated or cooled, are found to produce a bending of the whole bar, which is more the thinner its component parts. At the very surface of contact, and at a considerable distance on each side of that surface in thick bars, the principal effect must consist in what workmen would call wire-drawing the one metal, and upsetting the other. It is reasonable to think that this process must affect the properties of a balance so constructed, and cause it to deviate in the course of time from its original adjustment. This objection to the common expansion-balance appears to be obviated in Mr. Hardy's invention. The flexure of the brass takes place through its whole length, in a regular manner, and is in quantity but small; and the flexure in the reduced parts of the steel bar will be equally slight, if the thickness of that part be made to bear the same proportion to its length.

Hardy's balance has not the same faults

Hence, and upon the whole, it may be concluded that when once it is adjusted, it will not alter, and that in all changes of temperature it will be similarly affected, and will return to its original figure whenever the first temperature is restored.

It is easily made.

5th.—Artists will probably consider it as a desirable property of the present instrument, that the adjustments for temperature being in lines nearly parallel to the verge, will have no practical effect in deranging the adjustment for position.

I have the honour to be,
Dear Sir,
Your most obedient servant,
WILLIAM NICHOLSON.

Soho-square, March 7th, 1805.
To Charles Taylor, Esq.

A cer-

A certificate, dated March 6th, 1805, was received from Mr. Alexander Cuming, of Pentonville, stating that he had seen Mr. Hardy's expansion balance; that in his opinion it has considerable merit, and promises to act uniformly, steadily, and permanently.

Reference to the Engraving of Mr. William Hardy's Permanent Balance, Plate IV. Fig. 4, 5; expressing in inches and decimal parts of an inch, the dimensions of the several pieces.

Fig. 5. A A. Two globes which slide on the cylindrical stems of two upright levers, and are fastened by screws, by which the effect of the expansion is increased or diminished. **Figure and diagrams of the new balance.**

C C. Two equal and similar screws, by which the watch is adjusted to mean time.

D D. The verge or axis of the balance.

E E. The combination of the steel bar with the brass bars.

Fig. 6. S S. The steel bar, whose length is........	1.600
Its breadth..............................	0.232
Its thickness..........................	0.032
B B. Two similar and equal brass bars, in length each....................................	1.470
In breadth each,.........................	0.078
In thickness each.......................	0.032
Length of the two springs formed out of the steel bar..............................	0.030

VIII.

Description of an expanding Band Wheel to regulate the Velocity of Machinery. By Mr. Andrew Flint*.

The relative velocity of machinery connected by band is not variable.

In the usual method of connecting machinery, by a band running over two wheels or riggers; it is obvious that the relative velocity of the wheels is in the inverse ratio of their diameters; and these diameters always remaining the same, no alteration of the velocity can be obtained, but by a corresponding variation in that of the moving power applied.

Invention of the author to make it so.

To enable the artizan to regulate the velocity of his machinery at pleasure, the moving power remaining as before, or to retain the same motion, with an alteration in that of the applied force, is the purpose of the invention, the models of which are now laid before the Society. In this model are shewn two methods of attaining this desirable object; in both, the periphery of the band-wheel is divided into any convenient number of parts, according to the size of the wheels, (in this case twelve) which may be placed at any given distance from the centre of the wheel, (within the limits of the machinery) and thus, by enlarging the circumference of one band-wheel, while the other is equally diminished, to alter the relative velocity of each at will. These parts of the periphery, which I term V's, and are marked by the letters *i i i i i*, &c. Plate III. are confined to move in grooves, cut in the large wheels A and B, *Fig.* I. and II. in the direction of their radii, and are moved by a spiral thread in the small wheel C, which thread takes in the teeth of the racks on which the V's are fixed. A part of the shaft on which the wheel A is fixed, is made circular, to admit the small wheel C to turn round independently of the other, and thus to extend or contract the racks and V's in *Fig.* III.—*Fig.* IV. is a section of part of the rigger, in which the letters refer to the same parts as in *Fig.* I. and II.

* Society of Arts, Vol. XXIII. A premium of fifty guineas was awarded for this invention.

In

In the wheel D, the same effect is produced by the screws, *e*, *e*, &c. which are made alternately right and left handed, and turn with equal motions, by means of the equal bevil-wheels *f*, *f*, &c. fixed at their ends near the axis of the wheel. *Fig*. V. is a section of the same.

The wheel C, *Fig*. I. and II. is moved round the shaft *d* by the pinion *g*, on the axis of which is fitted occasionally a winch. The screws of the wheel D, *Fig*. III. may be also turned, by means of a winch applied to their projecting heads *h*, *h*, *h*. It is proper to notice that the number of the screws must always be equal.

ANDREW FLINT.

Goswell Street, London.

IX.

Account of a Discovery of native Minium. In a Letter from JAMES SMITHSON, ESQ. F.R.S. *to the Right Hon. Sir* JOSEPH BANKS, *K.B. P.R.S.**

MY DEAR SIR,

I BEG leave to acquaint you with a discovery which I have lately made, as it adds a new, and perhaps it may be thought an interesting, species to the ores of lead. I have found *minium* native in the earth. Character and habitudes of native minium

It is disseminated in small quantity, in the substance of a compact carbonate of zinc.

Its appearance in general is that of a matter in a pulverulent state, but in places it shows to a lens a flaky and crystalline texture.

Its colour is like that of factitious minium, a vivid red with a cast of yellow.

Gently heated at the blowpipe it assumes a darker colour, but on cooling it returns to its original red. At a stronger heat it melts to litharge. On the charcoal it reduces to lead.

* Philosophical Transactions for 1806.

In

In dilute white acid of nitre, it becomes of a coffee colour. On the addition of a little sugar, this brown calx dissolves, and produces a colourless solution.

By putting it into marine acide with a little leaf gold, the gold is soon entirely dissolved.

When it is inclosed in a small bottle with marine acid, and a little bit of paper tinged by turnsol is fixed to the cork, the paper in a short time entirely loses its blue colour, and becomes white. A strip of common blue paper, whose colouring matter is indigo, placed in the same situation undergoes the same change.

The very small quantity which I possess of this ore, and the manner in which it is scattered amongst another substance, and blended with it, have not allowed of more qualities being determined, but I apprehend these to be sufficient to establish its nature.

It seems to be produced by decay of galena.

This native minium seems to be produced by the decay of a galena, which I suspect to be itself a secondary production from the metallization of white carbonate of lead by hepatic gas. This is particularly evident in a specimen of this ore which I mean to send to Mr. Greville, as soon as I can find an opportunity. In one part of it there is a cluster of large crystals. Having broken one of these, it proved to be converted into minium to a considerable thickness, while its centre is still galena.

I am, &c.

JAMES SMITHSON.

Cassell in Hesse,
March 2d, 1806.

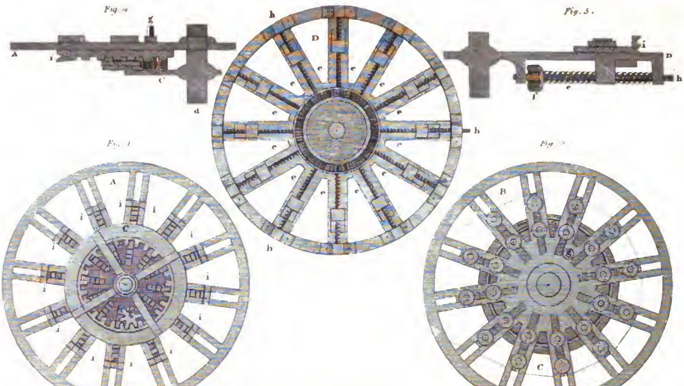
X.

*An Account of a new semi-metallic Substance, called Menacane, and its Ores. By the late G. Mitchel, M. B.**

The natural history of menacane has been little attended to.

SINCE the discovery of Menacane by Mr. Gregor, the distinguishing properties of the peculiar metallic substance it contains

* Irish Transactions, Vol. X.



Mr. Andrew Flint's Expanding Band Wheel

contains have been so fully developed, and satisfactorily ascertained, by the united exertions of Kirwan, Klaproth, Vauquelin, and Lampadius, that little is left to wish for, so far as the chemical characters are concerned. As an object of natural history, it has, as yet, been little attended to. It is therefore hoped, the following attempt, to supply in some measure that deficiency, so far as the present data allow it, will prove acceptable to the naturalist. It is scarcely necessary to observe, that I follow Werner's method most exactly: as it is to him that we are indebted for the successful vindication of Mineralogy, as an independent province in the fœderal state of natural history; and which acknowledges, in Chemistry, the powerful and indispensable ally, not the imperious and arbitrary law-giver.

Of the genus Menac we are already acquainted with five species or ores. It is, however sufficiently probable, that several new species will, at no distant period, be added to the list; and that this metal is more widely distributed, and more generally diffused, and plays, perhaps, a more important part, than is at present suspected. Five species.

MENAC, GENUS.

Tribe of Rutile .... { 1. Rutile. 2. Rutilite.

Tribe of Menacane.. { 3. Nigrine. 4. Menacane. 5. Iserine.

FIRST SPECIES.

RUTILE*.

Titanite of Kirwan.
Rutil of Werner.
Sagenite of Saussure.

Species I. Rutile.

EXTERNAL CHARACTERS.

The colour varies from light hyacinth to dark brownish red. Is found crystallized. 1. In right angled four-sided prisms, External characters.

* Probably the anatase of Hauy is a variety of Rutile —R. J.

prisms, acuminated by four planes, which are set on the lateral planes. 2. In six-sided prisms, which are said sometimes to exhibit a tendency to a six-sided acumination. 3. In acicular and capilliform crystals, whose regular shape is no longer determinable, and which are, moreover, strongly compressed.

The crystals are longitudinally sulcated, often very deeply; are commonly small, and very small, rarely middle sized. The acicular are often fascicularly aggregated: the capilliform crystals are often in a singular manner reticulated, the interstices forming equilateral triangles; exteriorly, shining and moderately glistening; interiorly, glistening; the lustre adamantine.

The principal fracture is foliated with a two-fold cleavage, cutting each other at right angles: the transverse fracture is imperfect and minute conchoidal. The fragments are cubical. It sometimes exhibits slender, columnar, distinct concretions. Is usually translucent, sometimes only translucent at the edges. Hard. Brittle. Gives a pale orange yellow streak. Is easily frangible. Heavy, in an inferior degree, about 4,200.

OBSERVATIONS.

Observations on Rutile.

The larger crystals, particularly those from Hungary, are often curved, have frequent transverse rifts, are sometimes broken entirely across, the ends removed to some distance from one another, and the interstices filled up with the substance of which the matrix consists: sometimes two crystals meet under an angle more or less obtuse, and are joined like the corner of a frame. The crystals are, moreover subject to great irregularities, are seldom fully crystallized, and, therefore, rarely acuminated; the four-sided prisms are often slightly rhomboidal; the six-sided prisms, from Hungary, are usually dilated, and seem composed of accumulated acicular crystals, from whence arise the columnar distinct concretions; the six-sided prisms, from France, are said to originate from the truncation of two opposite lateral edges of the four-sided prism; the capilliform crystals are sometimes coloured green, from chlorite earth. By some authors, this fossil has been said to resemble red silver ore; but the slightest acquaintance with the oryctognostical characters is sufficient to shew the difference; a geognostical character also furnishes

furnishes us here with an easy means of distinguishing this fossil from other ores of a red colour. Rutile is generally of cotemporaneous formation with its associated fossils; whereas red silver ore, red orpiment, &c. being formed in veins, are always of later formation than the rock on which they are seated. Some systematic writers have confounded it with rubellite, with which it has scarcely two characters in common.

CHEMICAL CHARACTERS.

Chemical characters of rutile.

Without addition, or even with phosphoric salts, it is infusible by the heat of the common blow-pipe; with borax or alkali, it affords a hyacinth red transparent glass; with the heat excited by pure air, it gives a milk white bead, and suffers a considerable loss of weight. It is insoluble in the mineral acids, before it has been melted with alkali, but yields readily to acid of sugar; is precipitable by acid of galls with a bright red, and by Prussian alkali with an handsome dark green colour. The method of analysis I shall omit, as belonging properly to mineralogical chemistry; the result has shewn that this fossil consists wholly of the calx of Menac.

GEOGRAPHIC DISTRIBUTION.

Geographic distribution of rutile.

This fossil has hitherto been discovered in but few places, and in moderate quantity, principally near Rosenau, in Upper Hungary; in Mount St. Gothard, in Switzerland; in Fischthal, in the high mountains of Saltzburg; near St. Yrieux, in France; in the province of Burgos, in Spain; in the forest of Speysart, near Aschaffenberg, in Franconia; at Beresooskoi, in Siberia; and Olapian, in Transylvania.

GEOGNOSTIC OCCURRENCE.

Geognostic occurrence of rutile.

The Hungarian rutile is found imbedded in a kind of quartz, passing into rock crystal, and forming nests in mica slate; it is therefore of cotemporaneous formation with the rock in which it lies. That from St. Gothard, in Switzerland, occurs partly in those drusy cavities, which are not unfrequent in granific mountains of high antiquity, lying in or upon the rock crystal, adularia, and foliated chlorite, with which those cavities are lined, and partly dispersed through,

or seated in, the scarcely perceptible clefts of one of those nameless chloritic rocks, which abound so much throughout the Alps in general. That from Aschaffenberg is said to occur in granite; that from Saltzburg is found imbedded in massive common tremolite. The rutile from Spain and Siberia is embedded in rock crystal. It would therefore appear that this fossil lays claim to great antiquity, the time of its production falling within the period of the earlier primitive rocks, and that the metal it contains probably surpasses, in that respect, tin, molybdæna, and tungsten, vieing even with iron and manganese *.

The above description has been chiefly taken from an attentive examination of the specimens of rutile existing in the best collections of Vienna and Saxony.

SECOND SPECIES.

RUTILITE.

Species II. Rutilite.

Calcareo-siliceous titan ore of Kirwan.
Titanit of Klaproth.

EXTERNAL CHARACTERS.

External characters.

The colour varies from brownish red to dark reddish brown. Has been hitherto found only crystallized in very rhomboidal four-sided prisms, acutely bevelled at the extremities, the bevelling planes set on the obtuse lateral edges. The crystals are small, and very small, seldom middle sized. Exteriorly, they are shining. Interiorly, glistening, with a resinous lustre. The fracture is imperfect and minute conchoidal,

* Von Buch has discovered rutile in layers of quartz, in clay slate (Thonschiefer), near Nühlbach, in Salzburg, in the vicinity of metallic layers, consisting of copper glance, copper pyrites, iron pyrites, nickel, and rarely native copper: also on the mountain Brennkogl, in the valley of Fusch; where it occurs in mica slate, either reticularly aggregated in rifts, or in acicular crystals, accompanied by those singular cylindrically aggregated crystals of foliated chlorite, in venules of almost coeval formation with the rock itself.—Buch's Geognostiche Beobachtungens—R. J.

Rutile has also been discovered by Von Humboldt, on the summit of a mountain near Caraccia, in New Granada, at the height of 1316 toises.—R. J.

choidal, passing into the uneven. The fragments are indeterminately angular, tolerably sharp edged. The transparency varies, from translucent, through translucent at the edges, to opaque. Is semi-hard, bordering upon hard. Brittle. Gives a greyish white streak. Is easily frangible. Not particularly heavy, approaching the heavy (3,500).

OBSERVATIONS.

The lateral planes meet alternately under angles of 135° and 45. From the foregoing fossil it is sufficiently distinguished by crystallization, fracture, inferior hardness, and specific gravity. From grenatite it may readily be discriminated, by the difference in crystallization, fracture, and sort of lustre.

Observations on rutile.

CHEMICAL CHARACTERS.

Before the blow-pipe it suffers no change, nor in the heat of a porcelain furnace, when exposed in an earthen crucible; but in a crucible of charcoal it melts to an imperfect black glass, owing to the partial reduction of the metallic contents. With considerable difficulty, and only by repeated digestion, marine acid dissolves a third part of the weight of this fossil, consisting partly of the menac contents. Klaproth, from whom these characters are taken, found it to consist of nearly equal parts menac-calx, silex, and lime, to which Vauquelin joins a large portion of iron calx.

Chemical characters.

GEOGNOSTIC OCCURRENCE.

In the mountains of Passau, this fossil is found imbedded in a coarse granular aggregate of felspar and hornblende, and felspar and actynolite; therefore belonging to the genus green-stone, and order of primitive trap. In Norway it occurs in rocks belonging to the same formation, in which the celebrated layers of magnetic iron ore lie, and is associated with hornblende, and several individuals of a tribe not as yet sufficiently examined and described, but which evidently constitute middle links between actynolite and hornblende, and to which the names arendalite and acanticone have been applied. Near Dresden and Brünn it is found dispersed through sienite; and at Galway, in Ireland, in an uncommonly beautiful porphyritic sienite. Hence it appears, that this

Geognostic occurrence of rutilite.

this fossil has only occurred in rocks belonging to primitive trap, or in sienite, the last crystallization which took place within the primitive period, and must therefore be considered as a later production than rutile. Here a consideration of the laws of crystallization countenances the observations on the order in which the primitive rocks follow one another. The rutile, consisting of few and simple elements of cotemporary origin, with a granite, in which rock crystal occupies the place of quartz, and adularia that of common felspar, sufficiently bespeaks a period, when the solution being purer and more tranquil, furnished an earlier and purer crop of crystals; while the confused and irregular crystallization of primitive trap and sienite, together with the greater impurity of the felspar, and very compounded nature of the hornblende and rutilite, indicate an inferior purity of the solution, and, consequently, later precipitation of the crystallized mass.

Species III. Nigrine.

THIRD SPECIES.
NIGRINE.
Nigrin of Werner.

EXTERNAL CHARACTERS.

External characters.

The colour is dark brownish black, passing into velvet black. Is found in larger and smaller angular grains, and pebbles. Externally, moderately glistening. Internally, principal fracture is glistening; the transverse fracture moderately glistening. Lustre, adamantine. The principal fracture is imperfectly foliated, with a single cleavage; the transverse fracture is flat, and imperfectly conchoidal. The fragments are indeterminately angular, and sharp-edged. Perfectly opaque. Semihard. Brittle. Gives a yellowish brown streak. Heavy, in a moderate degree (4,500).

OBSERVATIONS.

Observations on nigrine.

This fossil is readily distinguished from menacane, by its stronger lustre and superior hardness, the colour of the streak, and by its not being in the least magnetic; which also

also sufficiently distinguishes it from iserine and iron sand *. Being found in company with fragments of rutile of a dark colour, the latter has by most been confounded under the same denomination; but the red colour of the rutile, joined to its perfectly foliated fracture, with a two-fold cleavage, intersecting each other at right angles, and the thence resulting cubical fragments, distinguish it sufficiently from nigrine.

The present description is taken from a specimen I had the pleasure of receiving from Professor Jacquine the younger, of Vienna.

CHEMICAL CHARACTERS.

The nigrine is infusible per se by the blow-pipe: but, with the assistance of borax, it melts to a transparent, hyacinth red bead: to acid of sugar, it readily yields its medac contents, which furnishes the characteristic precipitate of this genus. Klaproth and Lampadius have given us the constituent ingredients, 8 or 9 per cent. menac calx, and 2 or 1 calx of iron. It is probable, however, that the proportion of menac calx is over-rated; it appearing evident, from the description accompanying the analysis, that there had been no care taken to select the nigrine from the grains of rutile which accompany it. Chemical characters of nigrine.

GEOGNOSTIC OCCURRENCE.

The nigrine has been hitherto found only at Ohlapian, in Transylvania, in alluvial hills, consisting of yellow sand, intermixed with fragments and bowlders of granite, gneiss, and mica slate, and from which gold is obtained by washing. This gold is the purest found in Transylvania; a circumstance sufficiently indicating, that it belongs to a different, and, consequently, earlier formation, than the usual Transylvanian native gold, which occurs there in clay porphyry, grey wacce, and grey wacce slate, and belongs to the brass yellow variety, from the considerable alloy of silver which it contains. In these stream works, the nigrine is Geognostic occurrence of nigrine.

* Genuine iron sand must not be confounded with magnetic iron ore in a *sandy form*, which usually passes under that name.

obtained

obtained at the same time with the gold, and comes to us intermixed with grains of rutile, oriental garnet, native iron, cyanite, and common sand; which renders it extremely probable, that this fossil, also, is a native of the primitive mountains.

Species IV. Menacane.

FOURTH SPECIES.

MENACANE.

Menachanite of Kirwan.

Menacan of Werner.

EXTERNAL CHARACTERS.

External characters.

Is of a greyish colour, inclining somewhat to iron black. Only met with in very small, flattish, angular grains, which have a rough glimmering surface. Internally, moderately glistening, with adamantine lustre, passing into the semi-metallic. The fracture is perfectly foliated, approaching to the slaty. The fragments are indeterminately angular, and sharp-edged. Perfectly opaque. Is soft. Brittle. Retains its colour in the streak. Easily frangible. Heavy, in a moderate degree (4,427).

OBSERVATIONS.

Observations on menacane.

This fossil has been said, but erroneously, to have much resemblance to iron sand, from which it may be easily distinguished by the fracture, lustre, and inferior specific gravity.

PHYSICAL AND CHEMICAL CHARACTERS.

Physical and chemical characters.

Menacane is attractable by the magnet, but much more weakly than iron sand, or magnetical iron ore; it is infusible by the common blow-pipe, or heat of a porcelain furnace, exposed in a coal crucible, but melts, when in contact with a clay one; it also melts quickly to a black bead, before a blow-pipe animated by pure air. The menac contents may be easily extracted by digestion with acid of sugar. Klaproth and Lampadius, about the same time, have shewn, that it consists of nearly equal parts menac and iron calces.

GEO-

GEOGNOSTIC OCCURRENCE.

This fossil has hitherto been only found, accompanied by fine quartz sand, in the bed of a rivulet, which washes the valley of Menachan, in Cornwall. The neighbouring mountains belong to the primitive order, in which, most probably, the menacane formerly constituted a superficial layer; but, by their decomposition, and consequent degradation, by means of rains and floods, the earthy parts have been carried off, and the heavier metallic fragments collected in the valley. Geognostic occurrence.

FIFTH SPECIES. Species V. Iserine.

ISERINE.

Iserine of Werner.

EXTERNAL CHARACTERS.

The colour is iron black, inclining a little to brownish black. Is found in small obtuse, angular grains, and in pebbles, with a somewhat rough, strongly glimmering surface. Internally, it is shining, with semi-metallic lustre. Fracture is more or less perfectly conchoidal. Fragments are indefinitely angular, and sharp-edged. Perfectly opaque. Hard. Brittle. Retains its colour in the streak. Is heavy, in a moderate degree (4,500). External characters.

OBSERVATIONS.

Of all fossils, this has the strongest resemblance to iron sand; into which, as Mr. Werner first observed, it actually graduates, but may be distinguished from it by the shade of brown in its colour; by its superior external, and inferior internal lustre; by its less specific gravity; but, chiefly, by being only slightly, and that by a powerful magnet, attractable. From nigrine and menacane, it differs sufficiently in fracture and lustre. This, as well as nigrine, was first considered as a particular species by Werner: both which determinations were afterwards confirmed by the analysis. Observations on Iserine.

 CHEMICAL

CHEMICAL CHARACTERS.

Chemical characters.

As in the foregoing species, the menac calx may here be readily extracted by acid of sugar, the residuum being dissolved in aqua regia: on the addition of tartarised tartarin, a lemon yellow powder falls to the bottom, which is tartarised menac; what remains in the solution is iron. Lampadius, to whom we owe the analysis, found that menac and iron are here in a decreasing proportion; the latter amounting to about 20 per cent. A late experiment has shewn him, that iron sand contains the same principles, but, probably, in an inverted proportion.

GEOGNOSTIC OCCURRENCE.

Geognostic occurrence of Iserine.

Hitherto this fossil has been only found in the high Riesen mountains, which separate Silesia from Bohemia, near the origin of the Iser, dispersed through the granitic sand which forms the bed of that river. To what order of rocks it owes its origin is uncertain; but its near affinity to iron sand, which is exclusively an inmate of the flötz trap formation, and the certainty, that this formation was formerly superstratified, at a great elevation, on the Riesen mountains, (as the remains, which form the Buchberg *, and occupy the Schneegruben, sufficiently testify,) render it highly probable, that this fossil, also, may belong to that formation; and, consequently, dates its origin from a much more recent period than the foregoing species of this genus †.

GENERAL REMARKS.

General remarks on me-

These are the only fossils of this genus, with whose cha-

* The Buchberg (which I enjoyed the invaluable opportunity of examining with my excellent and ever to be regretted friend) is the highest basalt hill in Germany, being 2921 feet above the level of the sea, and the highest basalt, except that small quantity lodged in the cavity of the Schneegruben, which is some hundred feet higher. The hill itself is elevated about 500 feet above the Iser, that washes its granitic basis, and the Iserine is found at some distance below. We could, indeed, discover no trace of it in the basalt of the present hill.—R. J.

† Mr. Gregor (as stated in Nicholson's Journal) has found, that menac is one of the constituent ingredients of basalt; a fact, which adds much to the plausibility of Dr. Mitchell's very ingenious supposition.—R. J.

racters

racters we are as yet sufficiently acquainted to say, with certainty, that they form distinct species. Between the three latter and iron sand, the intermediate transitions, as between all adjacent fossils, are, probably, innumerable. Were we to take analysis alone for our guide, it would multiply the species without necessity, and lose sight of the intentions of Nature, who does not confine herself to 5 or 10 per cent. of an ingredient; beside, a Klaproth has confessed, that it is not so much the identity and proportion of the ingredients, as the particular state of their combination, (which to us is perfectly unknown,) that determines the nature of the resulting fossil. In addition to those fully determined species, we have been favoured, by Klaproth, with the analysis of a menacaniferous ore, from Aschaffenberg; by Vauquelin, with that of another, from Bavaria; and, by Abildgaard, with that of a third, from Barboe, in Norway; all which differ from the foregoing species, and from one another, in composition, or in the proportion of ingredients; so that it is impossible to determine, with any probability, to what species they belong, from the want of an adequate external description, and account of their geognostic occurrence. nacane and its ores.

The masterly hand of Klaproth has further detected this metal, in the iron sand, which accompanies the hyacynths, &c. in Ceylon, and in some of the iron ores of Norway; and Lampadius has lately discovered it, in the iron sand of Hohenstein, near Stolpen, in Saxony, and in that found with the pyrope of Bohemia. Besides these, I have seen, in the imperial cabinet, at Vienna, and some few private collections, ores, said to come from Stiria or Carinthia, and from Bohemia, in which the menac calx probably abounded; as may be conjectured, from the strong shade of brown in the colour, together with the considerable adamantine lustre, both which are strongly characteristic of this genus.

Uses.

The use of this metal is, as will readily be supposed, from its scarcity, and the newness of its discovery, very confined. The rutile, indeed, was, for a length of time, employed to give a brown colour, in the porcelain manufacture of Sevres, near Paris; but, from the difficulty of communicating an equal tint by it, has been since abandoned. The rock crystal, inclosing capilliform crystals of rutile, has been

 employed

employed as a setting for rings. The precipitates, especially those from acid of sugar, may be employed as water colours; that, by acid of galls, affording a good tile red, and that, with Prussian alkali, an agreeable dark green. The latter, also, communicates a durable colour to silk, as my friend, Lampadius, assures me; perhaps, with proper management, it might be employed to furnish the so much wished for durable green for the printing of cotton. And, lastly, its close connection with some iron ores, and those exactly of the most superior quality, such as the ores of Norway and Stiria, leads naturally to the suspicion, that it may possess some favourable influence upon the manufacture of iron, and, therefore, well deserves the attention of future inquirers. Such are the principal circumstances, at present known, respecting this genus of fossils; time will, doubtless, here, as usual, find much to amend, to correct, and to supply.

XI.

On the Cultivation of Grapes. By Geo. Cumberland, *Esq.*

To Mr. Nicholson.

Sir, *Bristol, Jan.* 18, 1807.

Useful instructions for defending grapes, and giving them the advantages of sunshine, and the heat of a wall, &c.

Perceiving that practicable improvements in all the arts that benefit existence are sure to meet with a favourable reception in your Journal, I trust you will accept the following account of some practices that have lately been successful in the management of fruit trees, particularly the vine on the exposed wall.

Having last year come into possession of some south walls covered with vines that were said not always to ripen so well as might be expected, I was advised to cover them with glass, as the only sure means of securing a very considerable produce; but as that mode was too expensive to suit my circumstances, I resolved to make trial of less costly expedients, and at first turned my thoughts to those bell-glasses blown with a hole in the back, into which the young bunches are introduced very early so as to expand within the glass, and when ripe are severed at the stalk, and delivered at the wider aperture.

The

The objection to these, however, soon appeared to be, that, even at the glass-houses costing 2s. 6d. each, and upwards, it required at least five years to recover their cost, according to the value of the fruit; next, that in consequence of the hole made in the back they are uncommonly brittle; then, that they can only be applied at a very early season; and lastly, that their colour being obscure, they were on that account less likely to advance the maturity of the bunches (one only of which can be introduced to each) than materials more diaphanous. Finding therefore that to blow them of white glass would nearly double their price, I caused some white flasks of the best flint glass, of about one foot long, to be divided in two by means of the process with a hot coal, and thus I procured out of each flask two covers of the shape of half melons, each of which were capable of covering two bunches at least; of small ones three at a time; and buying them by weight, I found they stood me in only about one shilling each.

Useful instructions for defending grapes, and giving them the advantages of sunshine, and the heat of a wall, &c.

These segments I bound with packthread, by making a sling and a tie, so that they were easily attached to the wall by means of a nail, and kept from swinging by a cross thread or two, and thus I covered a great many bunches at all periods, commencing with them when about four or five inches long, and stopping the east side of the glasses with the fresh leaves as I picked them off; for, contrary to the usual practice, I exposed my bunches while green to the sun partially, and some entirely, at a very early period; at a very early period also I began to strip all the leaves from the wall, and to take away all extraneous growth; in fact, I suffered no leaf to remain that touched the wall from the time the vines first came into leaf until the period when the grapes were almost ripe, nor any bunch of grapes at any period to be totally excluded from the sun, laying them particularly open to his declining beams, and only securing them with what care I could from the too piercing rays of noon.

In this manner, under these clear glasses, I exposed several bunches of a sweetwater, growing on a buttress in an angle of about 80 deg. due south, to every ray of sunshine, except the direct ones, and not only did so, but I cleared every leaf away from the wall that approached within a foot each way of

Useful instructions for defending grapes, and giving them the advantages of sunshine and the heat of a wall, &c.

having nothing more profitable to grow in their estimation; for it is only a very few years since, the walls and some of the vines now remaining, that a Mr. Fry had at Axbridge a well bearing vineyard on the southern side of that romantic hill, on land not worth at any rate more than half a crown an acre; and I have known a sack of very early potatoes sold for eighteen shillings, that were in a similar situation raised in the natural bed on land of no greater value; and filberts it is well known will grow on most of our southern poor hill lands, almost without the hand of culture.

Thus much I have thought might be useful to many people to know who have vines, which, for want of understanding these methods, they suffer either to remain as unfruitful ornaments, or coolly contemplate the destruction of, scarcely ever affording them the least manure, and expecting a spontaneous product once in six or seven years without any care at all. For although we have many expensive treatises on the management of vines under glass, except Evelyn in his 'French Gardener,' we have few authors who shew the possibility of raising a good English vineyard fit to make wine from; and as nothing is so easy as to make good wine from quite ripe grapes, I trust, by facilitating that operation, I shall render some useful service to the British wine grower, and, at any rate, increase the value of many vine-covered walls.

Great advantages of wool as a defence for peaches, &c

And now, Sir, having taken up, I fear, but too much of your paper, I will only beg leave to add, as briefly as possible, that last year, for the first time, I used *coarse wool*, in the rough state, to screen my peach and apricot blossoms from the east winds, by tucking it into the east side of every bunch of bloom, instead of fern, laurel leaves, or broom; and that this afforded an effectual security to the fruit even after it was set; an improvement which has these advantages, that it is always at hand, is cheap, can be repeatedly used, gives no strokes to the wall in windy weather, and keeps up an even temperature in the night, while it makes less litter, gives less shade, and by being left on, encourages the growth of the fruit, by retaining the dews and securing the fruit stalk from the scorching reflection of the wall at noon.

Trusting

Trusting that its utility may prove an apology for the haste with which the paper is written,

I remain,
Sir,
Always your obliged humble servant,
G. CUMBERLAND.

Useful Notes and Observations respecting the Islands of Orkney and Shetland. By PATRICK NEILL, *A.M. Secretary to the Natural History Society of Edinburgh.**

THE circumstance of the shores of Norway being clothed with fir-trees†, is doubtless a strong analogical argument in favour of the practicability of raising timber in the Orkney and Shetland islands. **Norway abounds with trees. The Orkneys, &c. are bare.**

"In respect to the soil," (says the Bishop of Bergen‡), "it is not the good, rich and black earth, "that favours "the fir-trees; nor the clayey soil; but rather the gravelly, sandy, or moorish lands." This is an observation well calculated to inspire hopes of success.

Thousands of young fir-plants are cut, every spring, by the peasants of Norway, for food to their cattle. It would not probably be difficult, therefore, to procure quantities of saplings from that country. But if this were found to be too troublesome, it may be suggested that the ripe cones might be brought over (and these could easily be collected), and that the seeds might, by way of trial, be sown where the trees were intended to grow. This simple plan might **Whether plants could be had from Norway.**

* Extracted by permission from his "Tour through some of the Islands of Orkney and Shetland." The spirit of active industry and the consequent improvements in science, arts and manufactures in every part of our island, cannot be better shewn and promoted than by the travels of intelligent observers. Most of the subjects in the small book before us are of great national importance and interest, particularly at a moment when so many of our sources of prosperity are endangered.—N.

† The fir trees of Norway are, I find, the *fure*, or spruce, pinus abies (not the silver fir); and the *gran*, or pine, pinus sylvestris, well known by the name of Scots fir.

‡ Nat. Hist. of Norway, Vol. I. p. 143.

 possibly

possibly be found preferable to raising the plants in nurseries or gardens in the islands. We should, in such cases, adopt every approximation to the methods of nature. Pontopiddan even suggests, that instead of inserting the seeds in the soil, it would be better to hang the branches, containing the cones, upon poles at different distances, and to allow the seeds to drop out and sow themselves. At any rate, the seeds might be merely raked in. The experiment might be tried on any piece of dry rocky land (an acre or more), which could most easily be protected from the inroads of sheep or cattle, the exclusion of these being indispensable. The seeds might be sown very close; and if only one in ten or twenty were to vegetate, (and that is not a very sanguine expectation), a flattering foundation would be laid for ultimate success.

Remarkable fact that trees seem to prefer the west coasts.

Having mentioned this subject to Mr. James Hay at Gordon Castle, he observed to me, that "it is remarkable that trees thrive naturally on the *west* coast of Scotland, as well as on the *west* coast of Norway, in some places very nearly down to the sea side; while, in several places on the *east* coast of Scotland, they cannot be reared at all; and therefore whatever cause of difference may lie in the *soil**, it would appear that much is owing to *exposure*. The exposure to strong, sweeping unchecked winds, seems to be the chief obstacle to the raising of timber. Hills act upon the wind as a dam-dike does on a running stream, in producing considerable stillness or even calm upon the side from which the current flows. This consideration should induce planters to begin always at the bottom of hills, and extend their plantations gradually towards the sea. A hedge upon the side next the sea, though desirable, could scarcely perhaps be reared of any tree or plant. Hippophae rhamnoides (sea buckthorn) might be tried; but Sambucus nigra (elder bush) would probably be found preferable."

Larch, ash, sycamore, &c.

For the raising of larch, ash, sycamore, and others, nurseries should be established in the islands themselves; it being certain that plants resemble animals in becoming gradually habituated to particular climates and soils.

Salixes.

In places where Salix acuminata, S. arbuscula, aquatica, and

* Is it not a general law over the face of the globe that the west sides of N. and S. chains, or mountainous ridges, are most steep?—N.

and others grow, various willows might be cultivated, suited for wicker-work and cooperage. Salix fragilis or crack-willow would grow freely; it makes large shoots every season, and bears cropping admirably. It answers well for making crets, cradels, and large baskets. The name *fragilis* only intimates that the annual shoot is very easily detached from the trunk, the twig itself being very flexible and tough. Salix viminalis or common osier, also grows very freely, and is much in request by coopers. Salix Helix, or rose willow; S. triandra, or long-leaved osier; and S. vitellina or yellow osier, would doubtless succeed, and they are all employed in basket-making. To these might be added S. Forbyana or basket osier, for the nicer kinds of work; and S. Russelliana, which would be very useful not only for making crets and creels, but in tanning,—the bark being superior for this purpose perhaps to oak-bark. A decoction of it would form an excellent liquor in which to steep their herring nets.

Molucca Beans.

On the Molucca beans cast ashore on Orkney.

I have lately observed a paper "on the beans cast ashore in Orkney," in Philosophical Transactions 1696, No. 222, by Sir Hans Sloane. He mentions three kinds as pretty common: the Cocoon: the Horse-eye-bean; and the Ash-coloured nickar. The two former are the kinds which I got in the islands, in 1804. The cocoon of Sloane is evidently the seed of the Mimosa scandens of Linnæus, the Gigalobium of Brown's "Jamaica." It is the largest of the beans figured in Wellace's "Description of Orkney," 1693. 2. The horse-eye-bean of Sloane is distinctly the seed of Dolichos urens Lin.; the Zoophtalmum of Brown, who calls the seed, ox-eye-bean. This is the smaller bean figured by Wallace, and is easily known by the hilus or welt which surrounds it, and which gives it somewhat the appearance of a horse's or ox's eye. 3. The ash-coloured nickar is the seed of the Guilandina bonduc Lin. It is not so commonly found as the others. It is a perfectly round hard seed, little larger than a musket-bullet.

Herring-Fishery.

Very great

This immense field for industry,—this inexhaustible source of

political advantages of the herring fishery of Scotland.

of wealth;—has been often described; but still it is in a great measure neglected; at least we certainly do not derive from it those vast advantages which it is calculated to afford, and which it did, for a very long series of years, afford to the States of Holland. At a moment when we are listening to the eloquent and plausible, but I fear seductive and dangerous arguments of the Earl of Selkirk in favour of emigration, I cannot omit this opportunity of very briefly calling into view the extent and the value of this fishery, which, if duly prosecuted, would afford cheerful and profitable employment at home, to any number of those deluded men who are every year abandoning their native country, in quest of imaginary happiness and riches in the woods and fens of America;—and I presume it will at once be conceded, that ten or twenty thousand Scotsmen engaged in the Shetland herring-fishery, would, in this eventful period, be a much more agreeable object of contemplation to the mother country, than the finest imaginable settlement in Prince Edward's Island, or on the banks of the St. Lawrence.

Immensity of the shoals.

It is scarcely possible to form an idea of the immensity of the grand northern shoal of herrings which approaches the Shetland Islands every month of June. "The flocks of sea-birds, for their number," it has been observed, "baffle the power of figures:"

> ———Where the Northern Ocean in vast whirls
> Boils round the naked melancholy isles
> Of farthest Thule;—
> Who can recount what transmigrations there
> Are annual made? what nations come and go?
> And how the living clouds on clouds arise?
> Infinite wings! till all the plume-dark air
> And rude resounding shore, are one wild cry*.

"But the swarms of fishes, as if engendered in the clouds, and showered down like the rain, are multiplied in an incomprehensible degree. Of all the various tribes of fishes, the Herring is the most numerous. Closely embodied in resplendent columns of many miles in length and breadth, and in depth from the surface to the bottom of the sea, the

* Thomson.

shoals

shoals of this tribe peacefully glide along, and, glittering like a huge reflected rainbow or aurora borealis, attract the eyes of all their attendant foes*."

Let it not be thought that this swelling description exaggerates the amount of the shoals: let it be coolly considered that for more than a century the Dutch annually loaded above a thousand decked vessels out of this grand northern shoal, and yet that this immense capture never in any year sensibly diminished the number of herrings around Shetland, which, after these foreigners were glutted, regularly continued to press forward toward the islands in vast bodies, frequently crowding into every creek and bay! —which is proved by the Dutch.

The Dutch, it is well known, accounted this fishery their "gold mine." It seems generally agreed among authors, that it yielded them, for a long course of years, 3,000,000*l.* sterling yearly. Dr. Campbell, after premising that the value of the Dutch fishery has often been exaggerated, and that he will therefore give a "modest computation," proceeds thus: "It would however be no difficult thing to prove, to the satisfaction of the candid as well as critical inquirer, that, while it continued to flourish in their hand, they drew from their fishery out of the ocean washing the coast of Shetland, to the amount of two hundred millions sterling*." From 1500 to 2000 sloops were employed in fishing: this gave occasion to the freighting of 6000 more; and thus the herring-fishery gave employment and subsistence to above a hundred thousand persons†. Amount of the Dutch fishery.

Captain Smith, who was sent to Shetland so long ago as 1633, expressly to report on the Dutch fishery, says, "I was an eye-witness of the Hollanders' busses fishing for herrings on the coast of Shetland, not far from Ounst, one of the northernmost islands. Demanding the number of them, I was informed that the fleet consisted of 1500 sail, of 80 tons burden each, and abont 20 armed ships, carrying 30 guns a piece, as convoy." The conclusion drawn by the captain, is quite characteristic of a British sailor: it is stated with much spirit, and though his plan is not a practicable one, his language forcibly shews how strongly his mind was impressed with the vastness of this fishery, and the Report of Capt. Smith in 1633 on this fishery.

* Bewick, Introd.

† Political Survey, Vol. I. p. 69F.

‡ Ibid.

absurdity

absurdity of neglecting it: "If the King* would send out such a fleet of busses for the fishing-trade, being in our own seas, and on our own grounds, and all strangers were discharged from fishing in those seas, that the subjects of the three kingdoms only may have it, it would make our king rich and glorious, and the three kingdoms happy; not one would want bread,—and God would be praised,—and the King loved."

Former fishery at Shetland,

About half a century ago, the herring-fishery on the coast of Shetland was very successfully prosecuted by some English companies. But, through unaccountable mismanagement, it has for many years past been abandoned. At present, also, owing to the troublous state of the North of Europe, this fishery is more neglected by foreigners than at any period during the last two centuries. Very few Danes, Sweeds or Prussians, I understand, now make their appearance. The French and Dutch dare not. A few sloops from ports on the east coast of Scotland are scarcely worth mentioning.

—abandoned.

With respect to local position, the Shetlanders themselves are best situated for carrying on this fishery: but owing to poverty, the tenants or fishers are quite unable to engage in it: they can only take a few hundred barrels of the inferior kind of herrings which enter their voes in harvest. In summer 1804, a scarcity approaching to famine prevailed in Shetland; yet herrings, in countless myriads, were known to be off Unst. How deplorable to think that the people should starve while there was, at the same time, a "waste, at their doors, sufficient to feed half the human race!" The capital requisite for the purchase of sloops, nets, salt and casks, in order to an effectual prosecution of the fishery at sea, would, it is believed, exceed the ability even of most of the Shetland lairds.

Proposals for its renewal.

From Shetland, however, this fishery, if undertaken by English or Scots companies, could best be carried on. It would here be accompanied with least trouble and risk of delay, and with least expence. Shetland is near to the scene of the fishery: the Shetlanders are remarkably patient of fatigue in the fishing: they are accustomed to very sorry

* Charles I.

accommodation;

accommodation: and being habituated to indifferent fare, would not require that expensive victualling which is indispensable to an English crew.

The rules observed by the Dutch curers are now generally known*, and in some degree practised. But still it would probably be of considerable advantage if the influence of government were employed to encourage some fishing-families from Holland to settle in Shetland. A few Dutch curers thus dispersed among the British smacks, might prove exceedingly useful.

May it not be hoped that some opulent English and Scottish companies,—under the fostering care of a paternal Government,—will undertake this Shetland fishery on a great scale,—a speculation which if persevered in, would surely, in the event, become exceedingly profitable. The Hamburgh market alone would take off the produce of a hundred sloops, except the taste for Shetland herrings has declined in the north of Germany. There is a great demand for herrings from our West India colonies, for the food of Negroes; and the home consumption would surely not be inconsiderable. If every inhabitant of the island were to eat only two herrings in the year, it would open a market for the produce of another hundred sloops, even supposing them to fish with the greatest possible success. The herring fishery is an undertaking, indeed, of national importance, not merely as a source of wealth, but as an additional nursery for our navy.

The markets are great and extensive.

If this fishery were to be extensively carried on from Shetland, some additional villages would become necessary, and winter-employments would be wanted. The manufacture of herring-nets might properly and advantageously occupy many during the winter: and with this, might commodiously be joined the manufacture of lines for the cod and ling fishery.

To these very cursory and imperfect hints on the importance of this fishery, I shall subjoin a few remarks con-

Particulars by Dr. Halliday respecting the herring.

* They are printed in the Transactions of the Highland Society of Edinburgh, Vol. II. 328—345.

nected

nected with the natural history of the herring, for the principal part of which I am indebted to my friend Dr. Halliday of Edingburgh, (now Halesworth in Suffolk).

I am aware that Dr. Anderson, in his Agricultural Recreations, has rendered it highly probable that the herrings, instead of rendezvousing near to the North Pole, as was formerly imagined, only retire a little way from our coasts, or sink deeper in the sea, at particular seasons. He remarks, that the fishery commences sooner in some southern bays, than in others that are more northerly: that the return of the grand shoal to the northward is never observed: that from peculiarities in the shape and size of the herrings at different fisheries, it is evident that the herrings of the same breed, or partial shoal, return annually to the same shores: and, that they do not retire towards the North Pole to spawn, as was formerly imagined; but on the contrary, are taken on our coast, both when full of roe, and immediately after spawning, when the fry are seen.

They breed near the coasts,

This last observation of the Doctor's is undoubtedly correct. The fry is, at particular seasons, seen in the Orkney and Shetland seas in incredible numbers: it is then called the herring-soil, and is accompanied by thousands of the smaller gulls and divers.

—and grow very fast.

The growth of the fry is very rapid; it has been watched by Dr. Halliday, who informs me, "that on the western shores of the Isle of Mull, he has observed, in the months of March and April, the herring-spawn which was accidentally entangled by the cod-lines, to be vivified; the two eyes and head of the herring being then discernible; and that this spawn was raised by those lines only, which were set on the banks at some distance from the shore. In a fortnight, however, he observed the fry, about an inch in length, in great swarms close by the shore; and in six weeks they were three inches long."—Hence Dr. Hallidy concludes, that it is possible the herring may attain its full growth in one year, instead of requiring three, as Dr. Walker and others have supposed.

They do not go very far from land.

Dr. Halliday further informs me, that he has observed that the herrings leave the western shores of Mull when about six weeks old, and steer to the northward: but that they do not go many leagues from land, he considers as beyond

beyond doubt. He conceives that some place not far distant from the island of Unst may be their rendezvous or grazing-ground, (if we may be allowed the expression):—that during the harvest and winter they keep near the bottom, where they feed and grow to maturity: that in the spring they collect, rise to the surface, and begin to move off in various directions to the southward, for the purpose of spawning.

As already remarked, they do not deposit their spawn near the shore, but in the middle of the lochs or bays, or on the banks which are generally to be found at the mouths of the lochs. If, however, they are frightened from the spawning ground, they fly towards the shores, and are then full of roe; but they soon retire again, and do not return till freed from their load. They then range along the shores for some time, and at last retire towards the north, following the fry of the former years. —but deposit their spawn near the middle of bays.

It may be proper to add, that it is frequently observed on the western coast of Scotland, that a few weeks after the first shoal has left the lochs, a second shoal enters them, in full roe. This second shoal appears in the end of October or beginning of November: they deposit their spawn and leave the lochs as before. It is possible that the fry which leaves the coast in the beginning of May, may be the same that returns to it next year about the same period, and that these may proceed from the spawn deposited in the latter end of the season; while the fry of the June spawn having got off before the winter commenced, may return the following November;—thus allowing, from the depositing of the roe, to the maturity of the herring, eighteen months.

XIII.

Description of a very useful Bolt for Bookcase Doors. By Mr. Peter Herbert, *No.* 33, *Bow Street, Covent Garden**.

Properties of the bolt.

MR. HERBERT presented to the Society a model of his invention. He intended it for a library book-case bolt, to facilitate the opening of both doors at once, and to secure the same, without the trouble of bolting two bolts in the common way. It will do for wardrobes, French casements, or folding sash doors, and will also make a good sash fastening, if let into the bottom sash, with a small orass knob to slide as common; it would bolt in the frame by the side of the sash cord, both sides at once; and he can also make it answer sundry other useful purposes if required.

Reference to the Engraving of Mr. Peter Herbert's Bookcase Bolt, Plate IV. Fig. 2.

Description.

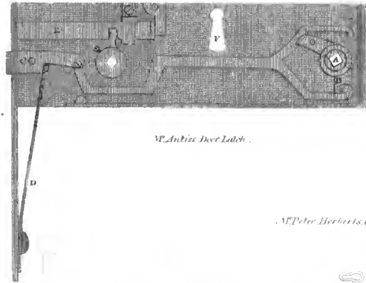
K, L, Fig. 3, represents the two stiles of the doors of a folding bookcase.

M, the key-hole of a lock with two bolts, which are more clearly shewn at Fig. 3, where the back of the lock N shews the two bolts of the lock pressing back a sliding piece, O; on the front part of this sliding piece in Fig. 2, two small friction rollers are placed at P, in the act of pressing against two levers, crossing on one common fulcrum R, to each end of which shorter levers, S S, above and below are connected by joints. These short levers act upon two long bolts, whose extremities are shewn at T T, having each a helical spring at V, V. In the state as engraved, the doors are locked and bolted.

On drawing back the bolts of the lock by means of the key, the helical springs V V press against the plates U U, through which the long bolts pass; they force back the long bolts and sliding piece O, and allow both the doors to open.

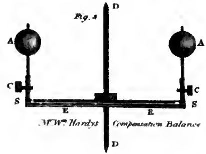
* Soc. Art. 1806. Ten guineas were given for this improvement.

Fig. 1.



Mr. Antiss Door Latch.

Fig. 4



Mr. Wm. Hardys Compensation Balance

Fig. 5

Mr. Peter Herberts Book Case Bolt.

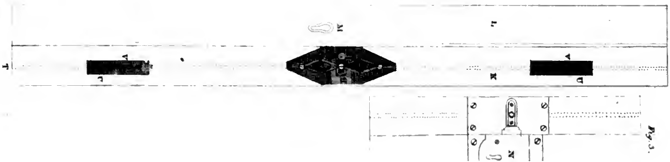


Fig. 2.

Fig. 3.

XIV.

Description of an improved Door Latch. By Mr. JOHN ANTIS*.

SIR,

I DO not doubt but that you are persuaded of the necessity of having a door-latch superior to, and not so liable to be out of order as those hitherto in use, in the door-locks of dining-rooms, &c. Some time ago, I made an attempt to contrive such a one, which I fixed into a small box by itself; I have now tried it for some years in my own house, during which time I never fonnd occasion to clean or to oil it. I at that time thought there would be a difficulty to introduce it into a mortise lock, in such a manner as to place the knobs and the key-hole symmetrically. That difficulty I have now overcome, and take the liberty to send you a pattern for your inspection. Imperfections of the door latches now in use.

My object has been to contrive a simple latch, as much as possible without friction, not more expensive than those hitherto in use, and capable of moving smoothly and easily without the necessity of cleaning and oiling, as long as the metal will last of which it is made. How far I have succeeded, I leave to the decision of the Society. Characters of the improvement.

I am, Sir,
Your humble servant,
JOHN ANTIS.

Fulneck, *April* 3*d*, 1804.
To CHARLES TAYLOR, Esq.

Reference to Plate IV. Fig. I.

A, shews the hole for the handle, which moves the follower and latch. B, the follower which draws back the latch, on turning the handle either way. C, the latch. D, the longitudinal spring, which throws out the catch of the latch when the hand is withdrawn. E, the small bolt, to secure the door internally. F, the key-hole, the bolt of the lock of which is not shewn, being placed above the key-hole. Description.

* Soc. Arts. This useful contrivance was rewarded with the silver medal.

SCIENTIFIC NEWS.

USEFUL NOTICES RESPECTING VARIOUS OBJECTS.

1. *Method of preventing Wet from being introduced into Rooms by Windows which shut together like folding Doors.*

Easy remedy to prevent rain being driven into apartments through the interstices of window frames.

A considerable inconvenience has been found from the wet penetrating, in rainy and windy weather, through the joints of those windows which have been called French windows, and are now much used. No accuracy of workmanship has been sufficient to remedy this evil; but, on the contrary, the closest joints have seemed rather more favourable to this effect than others less neatly made. Mr. Collinge, Engine-maker, of Lambeth Road, shewed me a very simple and easy remedy. Reasoning on the subject, he considered the close joint as a capillary interstice which would retain a continuous mass of water, much more disposed to be driven horizontally into the room by the action of the external air than to be conveyed downwards through a longer interval by its mere gravity. He has therefore enlarged the space for descending water by ploughing out a semi-cylindrical groove in each concave angle, from top to bottom. This small space, which is about one-tenth of an inch wide, occasions no deformity, and allows the water, as soon as it arrives there, to trickle down to the bottom of the frame, where it is conducted off by a similar concavity along the horizontal frame-work to any place of external discharge which may be made choice of. This easy and effectual cure for a nuisance which has destroyed the carpets, and occasioned puddles in very elegant rooms, and has apparently resisted all efforts to remedy it by close fitting, will, no doubt, be acceptable to many readers.

2. *Extemporaneous Printing Press, used by Country Comedians.*

I was informed, the other day, that it is the common practice of travelling companies of comedians to print their bills by laying the damped paper upon the form of letter previously inked, and to give the pressure by a wooden roller, clothed with woollen cloth. Many years ago I made experiments of this method, which I found very capable of affording impressions, by a light pressure. The form of letter must be disposed in a kind of frame, having its upper surface about one-thirtieth of an inch lower than the inked face, in order that the roller, being supported by the frame, may not be obliged to rise with much obliquity, upon the first letters; and that it may pass off, at the other end, with equal ease. If some such contrivance were not used, the paper would be cut, and the impression injured at the beginning and end of the rolling. The roller must be passed in the direction of the lines, or across the page; otherwise the paper will bag a little between line and line, and the impression will be less neat. In fact, the common method by the plattin, or flat surface which presses the whole at once, is best; but the engine is less simple.

Simple method of letter-press by means of a roller.

But as the arts of writing and of printing have incalculably extended the knowledge and powers of man, it may be allowed us to look forward to a time when communications shall be as much more rapid and effectual, compared with those of the present time, as ours are, compared with what they were before printing was invented. We may hope for a time when men shall confer more rapidly, concisely, perspicuously, and comprehensively by writing than they are now able to do themselves by articulated sounds. We may contemplate a period when by easy combinations of chemical and mechanical skill, the multiplication of numerous copies may demand scarcely more time and apparatus than is now required to write a single copy. And while we speculate on possibilities of this nature, which are far from being in the higher class of improbabilities, we may indulge a philanthropic hope, that when it shall be more easy to convey, distribute, and apprehend the results of philosophical and moral research, the short span of human life will be much less obscured by misery and accumulated suffering than

it is at present. Every step toward these ends is surely entitled to our notice.

3. *Art of Printing from Designs made upon the Surface of Stone.*

Prints or impressions from drawings upon stone.

I am not at present in possession of the history of an art which has been practised for some years in this town by several ingenious foreigners; namely, that of printing copies from designs made on the surface of stone. An eminent chemist informs me that the method is as follows:

Upon the surface of an hone, or close grained stone, designs are to be made in the stroke manner, with a pen, by means of an ink or pigment, made of a solution of lac in leys of pure soda, with a little soap added, coloured with lamp-black; or the designs may be made with a crayon of the same composition. I suppose that the proportions and manipulation would require some trials before perfect success would be obtained. When the design has been allowed to dry or harden for three or four days, the stone may be soaked in water, and its surface wetted. In this state if it be dabbed with Printers' ink from the balls, the ink will stick to the design, but not to the naked stone, and a copy may be taken from it by applying wet paper with pressure; whether of a rolling or screw press was not mentioned, but I suppose the latter to be preferable.

The advantage of this art appears to be that the print is given from an original, and not from a copy, as all engravings must necessarily be. It may also be considered as one of the means adverted to in our last article. For if a smooth stone, or a board of close wood, or perhaps some species of tile, or other prepared surface, could be written upon by an ink which, when speedily dried by the fire, or otherwise put into a state fit for use, could be made to afford impressions or copies by a simple roller, it would be easy to multiply bills, orders, notices, and an infinite number of other useful papers, to an extent which cannot at present be developed without much investigation and research.

3. *Gilding by means of Zinc.*

The same intelligent and active philosopher, whose name I forbear to mention only because I have not at this instant an opportunity of asking his permission, informs me that a coating of brass, formed by the precipitation of zinc upon copper, constitutes the surface of the beautiful gilt trinkets which at present abound in our shops, and are much superior in their appearance, and cheaper in price, than what were formerly made. New process of gilding.

The process is, Take of zinc one part and mercury twelve parts, with which make a smooth soft amalgam. It is better if a little gold be added. Clean the copper piece, or trinket, very carefully with nitric acid. Put the amalgam into muriatic acid, and add argol (by which name the crude tartar is denoted in the shops). Purified tartar will not do. Boil the clean copper in this, and it will be very finely gilt. Copper wire, thus coated, is capable of being drawn out to the fineness of an hair, though copper alone would not. This wire is used for making gold lace, and for epaulets and other similar articles.

The theory of the above process appears to resemble that of whitening pins; and its useful applications may probably be more numerous than those which have yet been adopted.

4. *Clock of the famous John Harrison, which does not require cleaning.*

Cummings, in his Treatise on Clock and Watch Work, mentions a clock of Harrison's which was constructed to go altogether without oil; but he does not say by what means the necessary lubricity of its moving parts was obtained. About two years ago I saw this clock in the hands of Mr. John Haley, Jun. The pivots of the wheels moved on friction rollers of considerable diameter; and the pivots of these rollers, or rather wheels, were brass, and moved in sockets of a dark coloured wood, which I think must have been lignum vitæ. Hence it should seem that the contrivance was reduced to that of rendering the surfaces of contact, where the sliding or friction was to take place, as slowly moving as possible, and in presenting a face which should afford a softish bed, having grease in its interstices. Similar to this is the practice of some mechanics, who make the bearing parts of the axis of a grindstone very smooth and round, and envelope them with a piece of bacon-skin, which is said to be very useful to keep away the sandy particles, and facilitate the motion for a long time without much wear. Harrison's clock without oil.

TO CORRESPONDENTS.

Extreme occupation during the concluding month of the year has prevented my searching into the authorities upon which De Lalande has established his comparison of the English and French measures, and also those from which he has deduced the measures of the earth's radii. I shall pay attention to the request of "A Constant Reader" in the next Number.

Mr. Walker's letter from Oxford arrived by the post; but not the pamphlet.

In answer to the inquiry of D. M. respecting a method of cleansing linen by the application of steam, as used by the French, I cannot point to any authentic account of a simple process of this kind, though I have been informed that the application of steam to piece goods, in a large digester, at a temperature considerably above 212°, is very effectual in cleansing, and promoting the bleaching process. This, however, seems fitter for the manufactory than the laundry. I am disposed to think that the method alluded to by D. M. is the Salzburg method, described in Van Mons's Journal, of which a translation is given at p. 127 of the tenth volume of our Journal, containing particular instructions how to carry it into effect.

I am sorry that a note of R. L. Edgworth, Esq. was not noticed earlier. Four lines from the bottom of page 82 of the last volume, the following should be inserted: "The number of teeth necessary for the wheel may be easily calculated to suit the measurement; so that the dial-plate may shew with sufficient accuracy five, or any other small number of miles."

Mr. R. L. E. speaks with commendation of Mr. Gilpin's crane in that volume; but remarks, that the groove which renders a common chain so much preferable to a rope for heavy burdens supported by tackles, has been long used.

I have just received the work of the Rev. P. Roberts, A.B.

Dr. Bardsley, Physician to the Manchester Infirmary, has committed to the press a Selection of the Reports of Cases, Observations, and Experiments, chiefly derived from Hospital Practice; including, among others, Clinical Histories of Diabetes (with Chemical Experiments on the Nature of diabetic Urine), Chronic Rheumatism, and Hydrophobia.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY

AND

THE ARTS.

MARCH, 1807.

ARTICLE I.

Experiments on Palm-Oil, by JOHN BOSTOCK, *M. D. Communicated by the Author*

To Mr. NICHOLSON.

THE appearance and physical properties of the substance called Palm-Oil, are sufficiently well known; but I believe its habitudes with different chemical re-agents, have never yet been attended to. Palm-oil,

Palm-Oil, as usually imported into this country, is of a deep orange-colour: its consistence is similar to that of butter, although perhaps, for the most part, a little harder and less unctuous. It has an odour peculiar to itself, somewhat aromatic, and not unpleasant. Its inflammability seems about equal to that of tallow; a cotton thread, inclosed in a quantity of it, was easily ignited, and burned with a clear, bright flame. —its obvious properties.

In order to ascertain the melting point of palm-oil, I heated a portion of it to the 100th degree, when it became perfectly fluid, and then observed the effect produced on the thermometer by its gradual cooling. When the mercury had descended to the 69th degree, the oil began to be slightly opake; Experiments respecting its melting point.

at 62°, it was completely so, and was of the consistence of honey: it continued to grow thicker until it arrived at 45°, the temperature of the room, when, although its fluidity was entirely lost, it still retained a degree of softness that it did not possess before the experiment.

The thermometer, as far as I could perceive, continued to descend without interruption during the whole period, and the oil seemed gradually to thicken in every part, without exhibiting any appearance of partial congelation. The inference which may be drawn from this experiment, seems to be confirmed by the following: Two equal quantities of the palm-oil were placed in similar jars; one portion was rendered completely fluid, and was then cooled down to 69°, when it began to assume a slight appearance of opacity; the other was heated to 65°, and was just beginning to melt. Both vessels were then plunged in a water-but of 100°: a thermometer inserted into each of them rose with equal rapidity, the first remaining 4° above the second. They were then removed, and the thermometers indicated an equally rapid decrease of heat, until they arrived at 48°, which was the temperature of the room. Equal quantities of palm and olive oil were heated, in similar jars, to the 100th degree, and then removed to a temperature of 45°: thermometers were inserted into each, and descended with equal rapidity.

Habitudes of Palm-oil with Alcohol,

Alcohol, at the ordinary temperature of the atmosphere, acts upon palm-oil in a very slight degree only. After remaining in contact for forty-eight hours, the fluid is perceptibly tinged of a yellow colour; and, by the addition of water, a slight degree of turbidness is produced, owing to the precipitation of a small quantity of palm-oil. By the application of heat, alcohol dissolves the oil more readily; a part of it is precipitated as the fluid cools, but a small quantity, about 1-75th of the weight of the alcohol, remains in permanent solution, and may be precipitated by water.

—and with sulphuric ether.

Sulphuric ether acts upon palm-oil with facility, at the ordinary temperature of the atmosphere, and produces a deep, bright yellow solution. The ether dissolves about 1-6th of its weight of the oil, and its solvent power is increased by heat. When water was added, the ethereal solution rose to the surface, and floated on the water without being decomposed. Palm-

Palm-oil is also readily dissolved by the oil of turpentine, at the temperature of the atmosphere.

Palm-oil has less attraction for pot-ash than olive-oil.

The action of caustic pot-ash upon palm-oil is similar to that which takes place between the alkalies and other bodies of an alcaginous nature. After being boiled together for some time, they form an opake and semifluid mass, miscible with water without decomposition, but which is slowly decomposed by the addition of an acid. In this latter case, the oil rises to the surface in small flushes, having lost its original colour and smell. The same effect, although in a less degree is produced by the action of ammoniac upon palm-oil. Palm-oil, however, exhibits less affinity for the alkalies than olive-oil.

Action of the sulphuric and nitric acids.

Palm-oil does not appear to be soluble in mineral acids. After being heated for some time in contact with them, it was left floating on the surface of the fluid, and, upon saturating the acids with an alkali, no precipitation was produced. The oil had, however, undergone a considerable change in its appearance and properties, from the operation of the sulphuric and nitric acids. In the former case, it had lost its specific smell; it was of a grey colour; and was considerably less unctuous than before the experiment. Upon being immersed in boiling water, it appeared to consist of two substances, of a white friable matter, which was diffused through the water, and had partly lost its oleaginous nature, and some small drops of a blackish oil. The effect produced by the sulphuric acid seemed to be similar to that which is described by Mr. Hatchett, in his valuable papers on the production of tanning*.

Oxidation by nitric acid.

The oil that had been heated in contact with nitric acid was also considerably changed: it was of a dirty colour, of a much firmer texture than in its natural state, and had acquired a smell resembling that of melted wax. The appearance of this substance seeming to coincide with the prevailing theory respecting the oxidation of oil, I was induced to examine how far it resembled wax in its chemical properties. First, in order to ascertain its melting point, a quantity of it was completely fused at a temperature of 110°. It was then gradually cooled; and when it had arrived at the 72d degree, it began to grow opake at the edges; at the 69th degree it had entirely lost its

R 2 transparency;

* Phil. Trans. 1805, p. 11, and alibi.

transparency; and, at 65°, it was become so firm, that the thermometer could with difficulty be removed from it. Hence it appears that palm-oil, by the action of nitric acid, is rendered less fusible, and that its fusibility is more nearly confined to a precise limit than in its natural state. Its solubility in alcohol appeared, however, to be rather increased; 100 grs. of alcohol dissolving very nearly 3 of the oil, two thirds of which were precipitated as the fluid cooled. The tendency of the palm-oil to unite with pot-ash was also considerably increased by the action of nitric acid. Equal quantities of the oxidated oil, and of the palm-oil in its natural state, were boiled with twice their weight of liquid pot-ash, nearly the whole of the oxidated oil was united to the pot-ash, and formed with it a thick saponaceous substance, while a considerable portion of the common palm-oil remained floating at the surface.

Effects of nitric acid on palm-oil continued.

Nearly the same effect was produced upon the palm-oil, by being boiled with nitric acid, by being digested in it for some weeks, at the temperature of the atmosphere, or by being precipitated, by the nitric acid, from its union with pot-ash. When the oil was digested without heat in the acid, its colour was first changed to a dirty green, next to a grey, and, lastly, was rendered nearly white. That, in these different processes, the oil was not united to the entire acid, but that a portion of the acid was decomposed, and its oxygene absorbed, I judged, because I found that the oil, after it had undergone the change, was not in any respect altered by being kept for some time in boiling water, nor did it impart to the water the least degree of acidity. This opinion was farther confirmed, by its union with pot-ash; if the oil had contained nitric acid, the addition of the pot-ash, instead of forming soap, would have reduced the oil to its original state.

Comparison with other oils

After having ascertained some of the leading properties of palm-oil, it appeared an interesting object of inquiry, to examine the relation that it bears to other substances, both of animal and vegetable origin, to which it exhibits some points of resemblance. I particularly refer to the expressed oil of vegetables, butter, tallow, spermaceti, the wax of the myrica cerifera, bees-wax, and the resin. The properties to which I particularly directed my attention, were the fusibility of the substances, and their habitudes with alcohol. The melting points

points of several of them, I had, on a former occasion*, taken some pains to ascertain with accuracy, on account of their having been so differently stated by authors of the first respectability. I now repeated the experiments with every possible care, and obtained the following results:

Tallow, heated to 120°, was perfectly fluid and transparent; at 99½°, a slight tendency to opacity was just perceptible; at 97°, it became very evidently opake round the edges; and at 90°, it was no longer transparent; at 89°, it had acquired a pretty firm consistence. The thermometer continued to descend during the process without any apparent interruption. A quantity of spermaceti was heated to the 120th degree, when it was perfectly fluid and transparent. The mercury descended to the 114th degree, when a slight opacity was perceptible at the lower edge; but it continued falling to 112¼°, when it became stationary. A film then formed on the surface, and very nearly the whole was rendered solid, when the thermometer began to descend again; but, upon agitating the part that remained fluid, the mercury rose to 112¼°. When the whole had concreted, the thermometer descended to the temperature of the room. Upon going through a similar process with myrtle-wax, heated to 120°, the opacity was observed to commence at the 116th degree; but the mercury did not become stationary until it arrived at 109¼°: here it stopped until the whole became solid, when the thermometer again began to descend. Bleached bees-wax showed a slight degree of opacity at 148°; but 142° or 141½° was the point where the mercury became stationary. The wax, however, retained a degree of softness at a much lower temperature. With respect to their fusibility, these bodies will stand in the following order—expressed oil, butter, palm-oil, tallow, myrtle-wax, spermaceti and bees-wax. I had not an opportunity of making the experiment upon expressed oil; but butter, palm-oil, and tallow are not only more fusible than the other substances, but they also agree in being liquified in a gradual manner; whereas the others pass more immediately from the fluid to the solid state, at one precise degree of temperature. With respect to the effects of alcohol, it is an opinion universally received, that expressed oil, butter, and tallow are not acted

Freezing-points of various oily substances determined.

Palm-oil melts at a low heat, &c.

* Nicholson's Journal, IV. 153 and seq.

Action of alcohol on expressed oils.

upon by it. This opinion, however, I found erroneous; not only was a small portion of each of them dissolved by being heated with alcohol, but even without the assistance of heat, a minute, yet very evident quantity, was taken up by the spirit. A part of the substance dissolved in the heated alcohol was precipitated as the fluid cooled, the remainder was separated by water, or by evaporation. The quantity was so small, that I found it difficult to ascertain its exact proportion.

Method of making the experiment.

The method that I pursued with respect to the spermaceti and the other kinds of wax, was to add them by degrees to the boiling alcohol, until a quantity remained undissolved. This would necessarily be melted, and would form itself into a small globule, which, when the fluid was become cool, might be removed. The fluid, together with that part of its contents which was precipitated by cooling, were then thrown upon a filtre, the weight of which was previously known, and the precipitated part being retained by it, it was easy to ascertain its amount. By weighing the fluid that passed through the filtre, and by permitting the alcohol to evaporate spontaneously, the solid contents that had been dissolved in it were ascertained. In this way were discovered both the whole quantity of the body that the alcohol dissolved, and that part of it which was continued in solution after the fluid had cooled.

Results of the action of spirit upon fat substances.

Proceeding in this manner, I found that 100 grs. of alcohol dissolved 52 grs. of spermaceti, half of which precipitated by cooling: 100 grs. of alcohol dissolved 2.134 grs. of myrtle-wax, 1.354 grs. being precipitated by cooling, and 1-8th gr. held in permanent solution. The same quantity of alcohol dissolved only .31 gr. of bees-wax, almost half of which was precipitated. The order in which these substances will stand, according to their power of resisting the action of alcohol, will be, olive-oil, butter, and tallow, nearly the same, bees-wax spermaceti, palm-oil, and myrtle-wax. The order of fusibility is, therefore, not exactly the inverse of the order of solubility in alcohol.

Their attraction for alkalies

The affinity of these several substances for the alkalies nearly follows the order of their fusibility, although not exactly so, tallow appearing to unite with caustic pot-ash more readily than with palm-oil.

Habitudes of Resins.

With respect to the resins, their fusibility and their solubility in alcohol, differ considerably in the different species; in general,

neral, however, they are less fusible, and more soluble in alcohol than any of the bodies mentioned above. It appears then, upon the whole, that palm-oil differs essentially in its physical and chemical properties from any substance that has hitherto been made the subject of experiment. Its fusibility is nearly similar to that of animal fat, while, in its chemical properties, it more nearly resembles the resins, at the same time that it differs from those bodies in not being soluble in nitric acid.

Liverpool, Feb. 14, 1807.

II.

Description and Use of a Calorimeter or Apparatus for determining the Degree of Heat, as well as the Economy attending the Use of various kinds of Fuel. By M. Montgolfier.

THE proper use of fuel is one of the most important objects in all the processes of the Arts, and more especially in Chemical Operations; and it is an object of no less utility, to determine the advantage and economy attending the uses of the various descriptions of fuel and the intensity of heat disengaged from the substances burned. Advantages of economy, &c. in fuel.

The same quantity of combustible matter of different kinds does not always afford the same degree of heat, and a longer or shorter portion of time will be required to disengage it from each combustible respectively. The success of an operation frequently depends on the rapidity with which it can be performed. Manufacturers, distillers, and cultivators must therefore consider it as an object of great importance to know what kind of fuel may be the cheapest for use, and what may be the proportion of a given quantity of the one compared with the same quantity of another, with regard to the effect to be derived from each; or, in short, what may be the most certain and easy method of determining the difference of the action of heat. The editors of the *Journal des Mines* speak with approbation of Mr. Montgolfier, for the instrument of which they have given a description, at the same time that they remark, Different combustibles vary in their effects.

that it very essentially differs from the instrument invented many years ago by Lavoisier and La Place.

Description of the Calorimeter.

The Calorimeter described.

Plate 5, exhibits a section of the Calorimeter of Montgolfier. A B C D is a vessel or box of tin, which might, with more economy and advantage be made of wood, sufficiently well constructed to hold water. In its cover A B, there is an opening *a b*; and so likewise, in the bottom, is an opening *e f*. Within this vessel is a small stove, *a b c d e f*, of plate-iron, or, which is better, of copper, carefully closed, so that no water can enter into it Its lower opening corresponds with that of the exterior vessel or box, *e f*. The other opening, in the other part is closed near *a b*, by a stopper which can be taken out at pleasure.

—It consists of a vessel having a stove within it, and a descending flue or chimney.

c d is a grate composed of iron wire, upon which the fuel is put, the ashes fall through the grate, and escape at the opening *g*.

Near *h i* is fitted a tube, *k k*, through which the smoke escapes by the opening *l*. This pipe must be made of iron or copper plate, sufficiently close to prevent the water from penetrating. *m m* is a pipe of plate iron, surrounding the last-mentioned in such a manner as that the water may be placed in the place between them. E is a reservoir, of which the cover, *r s*, can be taken off, in order to fill the apparatus with water.

o o is a pipe proceeding from the same reservoir, and communicating with the pipe *m m*.

n n is another pipe, which passes from *m m* into the vessel, for the purpose of introducing water, after it has passed through the pipe *m m*.

p is a cock, through which boiling-water may be suffered to escape; and *q* is another cock, by means of which the apparatus may be emptied when needful.

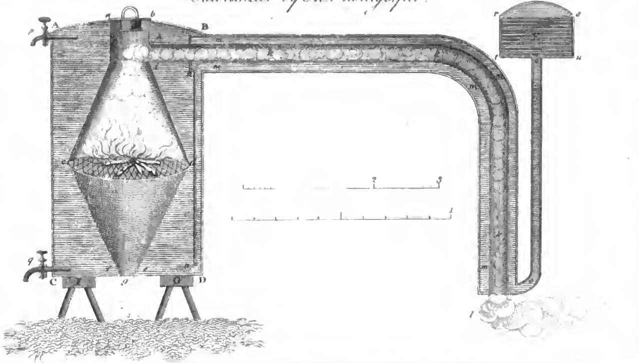
F G are the legs which support the apparatus.

Use of the Calorimeter.

Method of using it.

When it is required to determine the time in which different combustibles disengage, an equal quantity of heat, the reservoir *e* is to be filled with water. The fluid passes through the tube *o o*, rises through *m m*, and thence, by *n n*, into the vessel A B C D. A sufficient quantity must be poured to fill the

Calorimeter by M. Montgolfier.



the whole internal capacity of the vessel, which is easily known when the water does not descend below the line *t u*, or the most elevated station of that fluid in the apparatus: and the temperature must then be noted by a thermometer. A sufficient quantity of the fuel, for the purpose of an experiment, must then be taken; for example, wood cut into small pieces, and placed in the grate *c d*. After setting fire to it, the upper opening *a b*, of the stove, is to be closed, and notice taken of the time employed in raising the water to a certain heat; for example, that of boiling, which may be ascertained by a thermometer. At this period the fire is to be taken out, and the water and the apparatus suffered to cool to the first temperature at which the operation commenced. Another kind of fuel; for example, pit-coal or turf is then to be disposed on the grate *c d*, and the same observation made, after setting it on fire.

By observation of the time required to heat the water, the effects of the different fuels are known.

The greater or less rapidity with which heat is disengaged from the combustibles, will be known by comparing the times of the experiments respectively.

In order to find the difference in the quantity or weight of combustible matter of different kinds, proper to produce the equally-elevated temperature, it is necessary to take of one of the combustibles, for example, wood, a sufficient quantity, suppose one cubic foot. This is to be set on fire in the stove, after it hath been filled with water, and the temperature noted. The thermometer determines the period at which the water boils; and, at this period, the fire must be extinguished, and all the fuel taken out which remains on the grate. And when the whole has been brought to its first temperature, the process must be repeated with the other combustibles; for example, turf or pit-coal.

—and by the quantities of fuel consumed their economy is determined.

If, after the operation, the quantities of combustibles made use of be estimated at a medium price, it will be easy to show the cost of one compared with that of the other, and, consequently, what fuel is the least expensive.

We may also observe, that the external pipe *m*, may be made of wood; but if it be plate-iron or copper, it will be proper to cover it with a number of sheets of paper, forming a thickness sufficient to prevent the ready escape of heat.

The pipes, *k k* and *m m*, may be lengthened at pleasure, because

because a considerable portion of heat escapes through the aperture C.

The economical use of the present apparatus.

This apparatus may be used for different purposes; such as that of boiling water at a small expence. It is of great utility in domestic concerns. In order that its effect may be complete, the heated air ought to be deprived as much as possible of its caloric. The author, or perhaps the editors of the *Journal des Mines*, proceed to observe, that the cooled air being heavier than that of the atmosphere, causes the current in this kind of stove, and therefore they recommend that the descending tube should be made as long as local convenience will allow. It would not be needful to take notice of this oversight, if it were not accompanied with the practical deduction. The current is, in fact, produced by the rarefaction of that part of the air which ascends, and not by any increased density in the descending part, which, by the condition of the experiment, is, for the most part, in contact with hot water, and never colder than the surrounding atmosphere.

Error of the inventors.

III.

Letter from Mr. HUME, *of Long Acre, respecting the Carburetted Hydrogen Gas procured from Coals, by Dr.* CLAYTON, *early in the last Century.*

To Mr. NICHOLSON.

SIR,

AS an addition to the information already before the public, respecting the Hydrogen, or Carburetted Hydrogen Gas procured from Coals, it may not be improper to refer at once to an authority, beyond all others the most authentic and easy of access, I mean the Philosophical Transactions. In the 41st volume of that work, p. 59, there is a short paper on this subject, describing how the discovery originated, and some of the effects produced by this gas, or *spirit* of *Coals*. The paper appears to have been read before the Royal Society, in January, 1739, as, "a letter to the Hon. Robert Boyle, from the late Rev. John Clayton, D. D."

Dr. Clayton's Discovery of the inflammable Gas from Coals.

This

This letter is evidently a posthumous publication, and therefore may have been copied from that quoted by your correspondent Mr. Webster. However, lest there be any doubt, one being by *John*, the other by *James* Clayton, it is but fair to make both authorities known, in order that the merit of this discovery may no longer be disputed, nor claimed by any person living.

I am, Sir,
With much respect,
Your obedient Servant,
Long Acre, Feb. 10, 1807. JOS. HUME.

IV.

Curious Observations on the Wind, by ROGER ASHAM. *In a Letter from a Correspondent*

To Mr. NICHOLSON.

SIR,

IN the English works of Roger Ascham, which were reprinted at London, in quarto, anno 1761, under the care of James Bennett, I find a number of curious particulars; one of which I am tempted to send, for the information of your readers. In his Toxophilus, or School of Shooting, which relates to Archery, the subject is handled in a manner truly scientific and orderly, and such as is eminently calculated to show by what care and attention our ancestors obtained their pre-eminence in that celebrated art. The passage I now send you constitutes part of a dissertation on the effects which the direction and force of the wind, and the state of the air, may have in preventing the archer from striking his mark. In our time, these observations will be taken as bearing a more general relatinn to the mass of atmospheric phenomena. But I will not detain you with longer preface. I copy from p. 168, but do not follow the ancient orthography.

Introductory Letter respecting a Quotation from Roger Ascham.

I am, Sir,
Your obedient Servant,
R. B.

" The

Course of the wind.

"The wind is sometimes plain up and down*, which is commonly most certain, and requires least knowledge, wherein a mean shooter, with mean gear, if he can shoot home, may make best shift. A side wind tries an archer and good gear very much. Sometimes it blows aloft, sometimes hard by the ground, sometimes it bloweth by blasts, and sometimes it continues all in one; sometimes full side wind, sometimes quarter with him and more, and likewise against him, as a man with casting up light grass, or else, if he take good heed, he shall sensibly learn by experience. To see the wind with a man's eye, it is impossible, the nature of it is so fine and subtle; yet this experience had I once myself, and that was in the great snow that fell four years ago (1540). I rode in the highway betwixt Topcliffe upon Swale and Borowbridge, the way being somewhat trodden before by wayfaring men: the fields on both sides were plain, and lay almost yard deep with snow: the night before had been a little frosty, so that the snow was hard and crusted above. That morning the sun shone bright and clear, the wind was whistling aloft, and sharp, according to the time of the year: the snow in the highway lay loose, and trodden with horses' feet, so as the wind blew, it took the loose snow with it, and made it so slide upon the snow in the fields, which was hard and crusted by reason of the frost over night, that thereby I might see very well the whole nature of the wind as it blew that day, and I had a great delight and pleasure to mark it, which makes me now far better to remember it. Sometimes the wind would be not past two yards broad, and so it would carry the snow as far as I could see. Another time, the snow would blow over half the field at once; sometimes the snow would tumble softly, by and by it would fly wonderfully fast. And I also perceived that the wind goes by streams, and not together; for I could see one stream within a score of me, then the space of two score no snow would stir. But after so much quantity of ground, another stream of snow at the same time should be carried likewise, but not equally; for the one would stand still when the other flew apace, and so continue, sometimes swifter, sometimes slower, sometimes broader, sometimes narrower, as far as I could see. Now it flew straight, but sometimes crooked this way,

—observed on the surface of the snow.

— varying in the extent and velocity of its several streams

* From the context it appears that, by *plain up and down*, the Author means *directly to or from the mark*.

way, and sometimes it ran round about in a compass. And sometimes the snow would be lifted clean from the ground up to the air, and by and by it would be all clapt to the ground, as though there had been no wind at all; straightway it would rise and fly again. And that, which was the most marvellous of all, at one time two drifts of snow flew, the one out of the west into the east, the other out of the north into the east. And I saw two winds by reason of the snow, the one cross over the other as it had been two highways; and again, I heard the wind blow in the air, when nothing was stirred at the ground. And when all was still where I rode, not very far from me, the snow should be lifted wonderfully. This experience made me more marvel at the nature of the wind, that it made me cunning in the knowledge of the wind; but yet thereby I learned perfectly that it is no marvel at all, though men in wind lose their length in shooting, seeing so many ways the wind is so variable in blowing.

— and also in direction, &c.

Two currents of air at the same time

"But seeing that the master of a ship, be he never so cunning, by the uncertainty of the wind, loses many times both life and goods, surely it is no wonder, though a right good archer, by the selfsame wind, so variable in its own nature, so insensible to our nature, loses many a shot and game.

V.

Observations on the Marine Barometer, made during the Examination of the Coasts of New Holland, and New South Wales, in the years 1801, 1802, *and* 1803. *By* MATHEW FLINDERS, *Esq. Commander of his Majesty's Ship Investigator, in a Letter to the Right Honourable Sir* JOSEPH BANKS, *Bart. K. B. P. R. S., &c. &c. From Philosophical Transactions for* 1806.

[Concluded from Page 118.]

THE greatest range of the mercury observed upon the last coast, was from 29, 60 to 30, 36 at Port Jackson; and within the tropic from 29, 88 to 30, 30; whilst upon the south coast, the range was from 29, 42 to 30, 51, in the western part, where the latitude very little exceeds that of Port Jackson. It is to be observed, however, that these extremes are taken for very short intervals of time.

Observations and inferences to ascertain the correspondent changes of wind and weather, to be expected after change in the marine barometer.

My

Observations and inferences to ascertain the correspondent changes of wind and weather, to be expected after change in the marine barometer.

My observations upon the north coast of Australia are but little satisfactory, both because the changes in the barometer were very small in so low a latitude, and that very little more than the shores of the gulph of Carpentaria could be examined on account of the decayed state of the Investigator, which obliged me to return with all practicable expedition to Port Jackson. An abridged statement, however, of the general height of the mercury under the five following circumstances, will afford some light upon the subject, and perhaps not be uninteresting. 1st. On the east side of the gulph, and at the head, with the south-east monsoon, or trade wind. 2d. At the head of the gulph with the north-west monsoon. 3d. On the west side during the north-west monsoon. 4th. At Cape Arnhem under the same circumstance; and 5th. In the passage from Cape Arnhem, at a distance from the coast, to Timor, with variable winds.

In a memoir written by Alexander Dalrymple, Esq. F. R. S. respecting the Investigator's voyage, there is this general remark ;—" Within the tropics, the monsoon blowing on " the coast produces rainy weather, and when blowing from " over the land, it produces land and sea breezes." This I found verified on the east side of the gulph of Carpentaria, between November 3 and 16, which time was employed in its examination ; for though we had found the south-east trade to blow constantly on the east side of Cape York just before, and doubtless it did so then, yet in the gulph we had a tolerably regular sea breeze, which set in from the westward at eleven or twelve o'clock, and continued till seven, eight, or nine in the evening. Towards the head of the gulph, the trade wind, which blew at night and in the morning, came more from the NE, and the sea breezes more from north and NW, but without producing any regular alteration in the height of the mercury, whose average standard was 29,95 ; it never fell below 29,90 or rose above 30,04. At the head, the height of the mercury remained nearly the same, until the north-west monsoon began to blow steadily, about the 10th of December, two or three days excepted, when the day winds were from the south-eastward, and the mercury then stood between 29,80 and 29,85. At these times, however, there was usually some thunder and lightning about, signs of the approaching rainy

rainy monsoon, which may perhaps account for the descent of the mercury independently of the direction of the wind.

Observations and inferences to ascertain the correspondent changes of wind and weather, to be expected after change in the marine barometer.

2d. On the confirmation of the north-west monsoon, there was a change in the barometer at the head of the gulph, the common standard of the mercury being at 29,88; but during the times of heavy rain, with thunder, lightning, and squalls of wind, when amongst the islands of Cape Vanderlin, the mean height was 29,79. The north-west monsoon, after coming over Arnhem's Land, blows along the shore for a considerable part of the space between the Cape Maria and Cape Van Diemen, of Tasman; and during the examination of the parts so circumstanced, we sometimes had tolerably fine weather, and the mercury above 29,90; but the wind was then usually more from the north than when the mercury stood lower. As we approached Cape Maria, and the bight between it and the south side of Groote Eyland, the mercury stood gradually lower; and in the bight, where the north-west monsoon came directly from off the shore, although we had sea and land breezes with fine weather, according to Mr. Dalrymple's general position, yet the mercury was uncommonly low, its range being from 29,63 to 29,81: the average 29,74, below what it had stood in the very bad weather near Cape Vanderlin. These winds and weather, and the low state of the mercury, continued until we got without side of Groote Eyland.

3d. On the east side of Groote Eyland, and the west side of the gulph, northward from that island, we sometimes had sea and land breezes with fine weather; we had also two moderate gales of wind from the eastward, of from two to four days continuance each, with one of which there were heavy squalls of wind and rain; sometimes also, the winds were tolerably steady between north and west, with fine weather. During all these variations, the mercury never differed much from its average standard 29,90; and it seemed as if the increase of density in the air, from the wind blowing upon the coast, was equal to its diminution of quantity from the fall of rain and strength of the wind: and, on the other side, that the wind from over that corner of Arnhem's Land permitted the mercury to descend, as much as the fine weather would otherwise have occasioned it to rise.

Upon the north side of Groote Eyland, the mercury stood higher

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higher than usual for five days, and during this time the wind blew with more regularity from NW, the only exception being for a few hours in the afternoons, when it commonly sprung up from the NE in the manner of a sea breeze: the weather remained fine during these five days, and the height of the mercury averaged 29,94.

4th. In the neighbourhood of Cape Arnhem, the mercury usually stood about 29,90, whether the wind was fiom NW, NE, or east, if the weather was fine; but if by chance the wind shifted to the south side of west, off the land, it descended to 29,80 though the weather remained the same: and this was its standard during those times when strong gusts came from the NW accompanied with heavy rain, thunder, and lightning.

In this example, the wind from SW occasioned the mercury to stand lower than that from NW in the same weather; which is contrary to what was observed upon the south and east coasts; particularly on the former, where the south-west wind elevated the mefcury up to, and sometimes above 30,25.

5th. On March 6, 1803, we made sail off from the north coast, towards Timor, the north-west monsoon having ceased to blow at Cape Arnhem, and the eastwardly winds appearing to have set in; but we soon outran them, and had the wind so variable and light afterwards, that it took us twenty-three days to reach Coepang Bay, a distance of no more than 12° of longitude. The only two remarks I made upon the barometer during this passage were, that the common height of the mercury was 29,95 at those times that the wind remained steady for some hours, from whatever quarter it came, and about 29,85 when it was most unsettled; and that it stood higher, upon the average, after we had passed Cape Van Diemen, when the south-west winds, which blew oftenest, came from the sea, than it did before.

The medium height of the mercury, deducting the time between Cape Maria and Groote Eyland in the 2d example, I should take at 29,92, which, when the quantity of rainy squally weather, with thunder and lightning, is considered, is very high: the whole range of the mercury upon the north coast was four-tenths of an inch.

The

Observations and inferences to ascertain the correspondent changes of wind and weather, to be expected after change in the marine barometer.

The principal differences in the effect of winds upon this coast, from what they produced upon the south and east coasts, are, that a north-east wind raised the mercury as high, if not higher, than one from the SE; and that a north-west wind, where it came from off the sea and was moderate, was equal to either of them, and kept it up higher than the south-west wind did.

In order to have ascertained the full effects of sea and land winds upon the barometer, it was desirable to have learned, whether the south-east winds, which occasioned the mercury to rise highest upon the south and east coasts, would have left it at the medium standard, or made it descend upon the north-west and west coasts of Australia; but, unfortunately, the state of the ship did not permit me to determine this; for at the distance we kept from these coasts, in making the best of our way to Port Jackson, the accumulation of air over the shore, arising from a sea wind, or the contrary from a land wind, can scarcely be supposed to have much, if any effect. The principal winds we experienced between Timor and Cape Leuwen, in the months of April and May, were from SE and SW. The south-east wind prevailed as far as the latitude 25°, and the mercury stood at first with it at 29,95; but as we advanced southward, it rose gradually to 30,25, nearly in the same way as it had before descended on the east side of Australia, when we steered northward in the month of October. This wind was succeeded by an unsteady northwardly wind, which brought the mercury down to 29,90; but on its veering by the west to SW it rose fast, and fixed itself about 30,32: we were then drawing near Cape Leuwen.

As far as this example can be admitted in proof, it appears, that a wind from the SW has an equal, if not a superior power to one at SE in raising the mercury upon the west coast; which was not the case upon the south, and still much less upon the east and north coasts, where the south-west wind caused it to fall. Winds from the northward caused the mercury to descend, as I belive they always will in the southern hemisphere, if not obstructed by the land; but upon the north coast, we have seen the mercury stand higher with it than almost any other.

Upon a summary of the effects of the same winds upon the different

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different coasts of Australia, as deduced from the above examples, the following queries seem to present themselves:

Why do the winds from north and NW, which cause the mercury to descend and stand lower than any other upon the south and east coasts, and also in the open sea, and in the south-west bight of the gulph of Carpentaria, make it rise upon the outer part of the north coast, with the same, or even worse weather?

Why should the north-east wind, which occasions a fall in the barometer upon the south coast, considerably below the mean standard, be attended with a rise above the mean upon the east and north coasts?

The south-east wind, upon the south and east coasts, caused the mercury to rise higher than any other; why should it not have the same effect upon the north coast, and upon the west?

How is it that the south-west wind should make the quicksilver rise and stand high upon the south and west coasts,—should cause it to fall much below the mean standard upon the east coast,—and upon the north, make it descend lower than any other, with the same weather?

The answer, I think, can only be one; and it seems to be sufficiently obvious.

The cause of the sensibility of the mercury to winds blowing from the sea and from off the land, may perhaps admit of more than one explanation; but the following seems to me to be direct, and tolerable satisfactory. The lower air, when brought in by a wind from the sea, meets with resistance in passing over the land; and to overcome this resistance, it is obliged to rise, and it will make itself room by forcing the superincumbent air upwards. The first body of air, that thus comes in from the sea, being itself obstructed in its velocity, will obstruct the second, which will therefore rise over the first in like manner to overcome the obstruction; and as the course of the second body of air will be more direct towards the top of the highest part of the land it has to surmount, than the first was, so the first part of the second body will arrive at the top, before the latter part of the first body has reached it; and this latter part will be able to pass over the top, being kept down by the second body and the successive stream of air, whose velocity is superior to it. In this manner, an eddy, or body of compressed, and comparatively inactive air will be formed, which, at first, will occupy all the space below a line drawn

drawn from the shore to the top of the highest land: but, almost immediately, the succeeding bodies of air, at a distance from the shore, will feel the effect of the obstruction; and being impelled by those that follow them, will begin to rise, taking their course for the top of the highest land, before they come to the shore; by which means, the stratum of lower air will be deeper between the top of the land and the shore, and to some distance out from it, than it is either upon the mountains or in the open sea. If this is admitted to be a necessary consequence of a wind blowing upon the shore from the sea, it follows, that the mercury ought to stand something higher when such a wind blows, whether it is from the south or any other quarter, than it will with the same wind where it meets no such obstruction; and the more direct it blows upon the coast, and the higher the land is, (all other circumstances being equal,) the higher ought the mercury to rise. On the other hand, when the wind comes from off the hills, this dead and dense air will be displaced, even from its hollows under the highest land; both on account of its own expansion, and because its particles will be attracted by those of the air immediately above, which are taking their unobstructed course out to sea; and thus the air over the coast will resume its natural state with a land wind.

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In order to appreciate duly the effect of sea and land winds upon the barometer, in the preceding examples, it is necessary to be recollected, that in the southern hemisphere, a wind from the south has a natural tendency to raise the mercury in the open sea, and one from the north to depress it; probably, from the superior density of the air brought in by the former; therefore, if the mercury rises quicker and higher with a south wind upon the south coast, then it does with a north wind upon the north, it is not to be at once concluded, that the effect of the wind as coming from the sea, is less upon the north coast; for it has, in the first place, to counteract the tendency of the mercury to fall with a north wind; and, in some cases, its effects as a sea wind may be as considerable, relatively to the latitude, where there shall be no rise in the barometer, as upon the south coast it might where a considerable one took place. The same thing may be said of the winds from the east and from the west; for where the vicinity of land is out of the question, the former generally causes an ascent (from what principle, I leave others to determine,) and the latter a descent in the barometer, and I

 believe

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believe this extends to both hemispheres, and all climates. The wind from SE then, which combines something more than half the power, both of the south and of the east wind, will raise the mercury higher than any other on the south side of the equator, and the wind from NW permit it to fall lower, independantly of their effects as sea and land winds; and this allowance requires to be first made upon them: the south-west aud north-east quarters should be equal where there is no land in question, and of a medium strength between the power of the south-east, and the deficiency of the north-west wind.

I leave it wholly undetermined, whether the effects of sea and land winds upon the barometer, as above described, extend beyond the shores of the country where these observations were made, and to about one hundred leagues of distance from them; but it seems not improbable, that they may be found to take place near the shores of all countries similarly circumstanced; that is, upon those which are wholly, or for the most part, surrounding by the sea, and situated within the fortieth degree of latitude. In colder climates, where snow lies upon the ground during a part of the year, the wind from off the land may perhaps be so cold, and the air so much condensed, as to produce a contrary effect; but this, and the prosecution of the subject to other important consequences, I leave to the philosopher; my aim being only to supply my small contribution of raw materials to the hands of the manufacturer, happy if he can make them subservient to the promotion of meteorogical science.

I will conclude with stating a few general remarks upon the barometer, such as may be useful to seamen.

It is not so much the absolute, as the relative height of the mercury, and its state of rising and falling, that is to be attended to in forming a judgment of the weather that will succeed; for it appears to stand at different heights, with the same wind and weather, in different latitudes.

In the open sea, it seems to be the changes in the weather, and in the strength of the wind, that principally affect the barometer; but near the shore, a change in the direction of the wind seems to affect it full as much, or more, than either of those causes taken singly.

It is upon the south and east coasts of any country in the southern, or the north and east coasts in the northern hemisphere

sphere, where the effect of sea and land winds upon the barometer is likely to be the most conspicuous.

Observations and inferences to ascertain the correspondent changes of wind and weather, to be expected after change in the marine barometer.

In the open sea, the mercury seems to stand higher in a steady breeze of several days continuance, from whatever quarter it comes, provided it does not blow hard, than when the wind is variable from one part of the compass to another; and perhaps it is on this account, as well as from the direction of the wind, that the mercury stands higher within the tropics, than, upon the average, it appears to do in those parallels where the winds are variable and occasionally blow with violence.

The barometer seems capable of affording so much assistance to the commander of a ship, in warning him of the approach and termination of bad weather, and of changes in the direction of the wind, even in the present state of meteorological knowledge, that no officer in a long voyage should be without one. Some experience is required to understand its language, and it will be always necessary to compare the state of the mercury with the appearance of the weather, before its prognostications will commonly be understood; for a rise may foretel an abatement of wind,—a change in its direction,—or the return of fine weather; or, if the wind is light and variable, it may foretel its increase to a steady breeze, especially if there is any easting in it; and a fall may prognosticate a strong breeze or gale, a change of wind, the approach of rain, or the dying away of a steady breeze. Most seamen are tolerably good judges of the appearance of the weather; and this judgment, assisted by observation upon the quick or slower rising or falling of the mercury, and upon its relative height, will in most cases enable them to fix upon which of these changes are about to take place, and to what extent, where there is only one; but a combination of changes will be found more difficult, especially where the effect of one upon the barometer is counteracted by the other: as for instance, the alteration of a moderate breeze from the westward with dull, or rainy weather, to a fresh breeze from the eastward with fine weather, may not cause any alteration in the height of the mercury; though I think there would usually be some rise in this case. Many combinations of changes might be mentioned, in which no alteration in the barometer would be expected, as a little consideration, or experience in the use of

this instrument, will make sufficiently evident; the barometer alone, therefore, is not sufficient; but in assisting the judgment of the seamen, is capable of rendering very important services to navigation.

VI.

Analysis of the Substance known by the name of Turquois. By M. Bouillon Lagrange.

Many Mineralogists have placed the turquois among calcareous bodies and the genus called opake; and others, on account of their blue or green colour, have classed them among the ores of copper.

Chaptal's account of the Turquois stone

"The turquoises, (says M. Chaptal) are merely bones coloured by the oxides of copper. The colour of the turquois often passes to green, which depends on the state of the metallic oxide; the turquois of Lower Languedoc emits a fetid smell by the action of fire, and is decomposed by acids; the turquois of Persia emits no odour, and is not attacked by acids. Sage suspects that the osseous part is azatized in those left."

Places where they are found

Many turquoises are found in Persia, but none in Turkey, as its name seems to imply. They are obtained from two mines: the one, called the Old Rock*, at three days journey to the north-west of Meched, near Nichaburgh; the other, at five days journey, is called the New Rock. The Turquoises of this last place, are of a bad blue, inclining to white, and are therefore cheap. But since the close of the last century, the King of Persia has prohibited the Old Rock to be explored, except for himself; because the workmen of the country working only in wire (*en fil*), and not being acquainted with the art of enamelling on gold, they made use of it for the mounting of sabres, poignards, and other tools, turquoises of this mine, instead of enamel, by cutting and setting them in different forms.

I shall add some other details, extracted from different works;

* Annales de Chymie, LIX, 180.
La Harpe's Abrégé des Voyages, VI, 507.

works; in which I am indebted to the kindness of the celebrated mineralogist M. Haüy.

Turkis (turquois) Reuss, page 511, part 2, vol. 3.

The Turquois has always been considered as the tooth of an unknown animal, of which the sky-blue colour depends on oxide of copper; or, according to others, on oxide of iron; which has caused it to be ranked in the calcareous order, and sometimes in that of copper, as animal petrefaction (odontalite.) Various accounts of the turquois, by different authors.

Lommer, in the *Abhandlungen einer Privat-Gesellschaft in Bœhmen*, 2 vol. page 112, 118, thinks the turquois is a produce of art. He asserts, that a tooth found in the neighbourhood of Lissa, in Polomie, being exposed to a strong heat in the muffle of an assayer's furnace, became converted into a turquois; and he recommends the heat to be very gradually raised, for fear the tooth should fly in pieces.

Bruckman gives a complete history of all that has been written from Pluds to Lommer, on the turquois. He mentions mount Caucasus as a place of origin, at the distance of four days journey from the Caspian Sea; where, according to Chaidin, this stone is dug up. It is likewise in Persia, Egypt, Arabia, and in the province of Samaveande. Supposition that the turquois is not an animal substance.

Dambsy brought it from Peru; some of them contained native silver.

The occidental turquois is found in France, at Simore, in Lower Languedoc, in Bohemia, in Siberia, and in Hungary.

Demetrius Agaphi, who visited the place where the turquois is found, near Chorasen, in the neighbourhood of the town of Pishepure, relates, in the fifth volume *der Nordischen Beiträge*, 1793, page 261, that the turquois is found in a stone as its matrix, in masses and small points; and that it might be considered as a peculiar mineral, which has the same situations as opal, the chrysophane, and the resiniform quartz.

Mr. Bruckman, in *Crell's Chemical Journal*, 1799, vol. 2, page 188 to 199, thinks, from the nature of its position at Chorasan, and after the analysis at Lametz, that the turquois is not a petrefaction of parts of animals, but a particular mineral.

Lonsitz obtained from it, by analysis, much clay, a little copper, and iron; but neither lime, nor phosphoric acid *.

S 4 According

* I do not know whether the substance analyzed by M. Lowitz,

The oriental turquois supposed not to be a petrefaction.

According to Meder, the oriental turquois is found in a primitive argillaceous schistus, of a grey bluish, or black greyish colour, which excludes all supposition of petrefaction. Graphic schistus, and quartz, are found in the same place. In the argillaceous schistus, the turquois is found disseminated; and it is the same with quartz, and the graphic schistus.

To remove every idea that the turquois cannot be considered as malachite, or green copper (Kupfergrün), Meder has given the following character:

Its colour is apple green greyish; when it begins to soften, it is decomposed, and assumes a mountain-green colour; when completely decomposed, it is of a yellowish white, green, and near straw colour.

It is commonly found disseminated in small superficial parts, and seldom in masses; its interior structure is dull, or scarcely sublucid; its fracture compact, and the fragments irregular with sharp edges, opake when it is decomposed, and more or less transparent at the edges. Its hardness varies according to the degrees of its decomposition, it is easily broken, and its specific gravity, according to Curwan, is between 2,500, and 2,908.

The turquoises are not all of equal hardness: this must be attributed to the differences of the boney substances which constitute their base. The degree of petrefaction must also influence this property.

The turquois, in the solid form, is sometimes mixed with the brown earthy oxides of copper.

M. Meder infers, from all these characters, that the turquois ought to be placed between the opal and the chrysopaze, with which it appears to agree by the varieties of green.

Cuivier considers the turquois, as fossil teeth, coloured by copper.

Lastly, the celebrated Cuivier, in the *Journal de Physique*, page 263, vol. 52, thinks that the turquoises, namely, those which are found near Simore, in Languedoc, and near Trévoux, are the cuperferous teeth of an animal, resembling that which has been found near the Ohio, or the mammoth of the English and Americans; the carnivorous elephant.

Mr. Reaumur alone has given some detail respecting the mines of turquoises, and the nature of the substances there found.

His

ought to be considered as a turquois, or as a particular mineral; but I am more disposed to think it was not as in all the turquois. I have examined, and found lime and phosphoric acid. D. L.

His memoir is printed among those of the Royal Academy of Science, for the year 1715, which may be consulted for every thing which bears relation to the situation of the mines, and the extraction of the turquoises. Reference to the mines of turquoises, &c.

With regard to the experiments made by the author, in order to discolour these substances, though not very conclusive, it appears to me, nevertheless useful, to bring together these facts along with the means which I have employed to ascertain the nature of the stone. I shall first present a few fragments of this part of Reaumur's memoir.

The colouring matter, says the author, which fills the cellules of the turquois, and which afterwards tinges the whole stone, is, no doubt, a particular substance; but is it a simple mineral matter, like cobalt, or the material from which azure and saphor are made, from which the finest blue of porcelain, and pottery is served; or, is it a metallic matter? I have not been able to satisfy myself in this respect. Reaumur's observations on the colouring matter.

I at first suspected that our turquoises might probably derive their colour from copper. This metal is capable of affording a blue, and a green . . . . . . But I have found that the turquoises may be extracted like that of coral; of all the solvents, which I have used, distilled vinegar succeeded the best. If a thin piece of turquois be steeped in this vinegar, its angles, after an hour or two, become white; and in two or three days, the whole of the upper surface of the stone, and even its internal parts, assume the same colour. It was taken up by vinegar.

Vinegar, while it extracts the colour, likewise dissolves the stone; it is always covered with a kind of white cream, composed of parts which have been detached. Juice of lemons likewise dissolves this kind of stone, but it only weakens the colour; and that which is found under the kind of cream, we have described, is blue, when the stone has been put into this liquid.

As to aquafortis, and aquaregia, they are not proper to extract the colour from our turquoises; they very speedily dissolve the whole substance of the stone, but they afford the means of distinguishing the Persian turquoises from those of France. Aquafortis does not act upon those of Persia; whence it follows, that these two kinds of stone, though similar in appearance, are nevertheless of a very different nature; it would be wrong, however, to draw a consequence to the disadvantage Aquafortis, &c. applied to these stones.

tange of our stones, by concluding that they have less tenacity.

Aquaregia likewise acts differently upon these two kinds of stone. It totally dissolves ours, and it reduces those of Persia into a kind of paste, more whitish than the turquois was, but which is not, nevertheless, deprived of all its blue colour.

Turquoises lose their colour, and their value by time.

In general, this kind of stone has a singular defect; namely, that, without the assistance of any other agent than that of time, their colour changes: insensibly their blue assumes a shade of green, they become greenish, and at last green; whereas the colour of other precious stones is unchangeable. When the turquoises have become green, they are no longer of any value; the convention of society has placed them in no estimation whatever with that colour.

Chemical Examination.

Characters of the turquois.

Physical characters. Specific gravity, 3.127.—Colour, light green and blue; surface, smooth or polished; hardness, such as slightly to scratch glass; difficult to be pounded; powder, greenish grey; fracture, polished.

Habitude with the blow-pipe.

Chemical characters. Before the blow-pipe, it loses its colour, and becomes of a greyish white, but does not melt.

Heated in a crucible of platina, it acquires the same colour, but becomes friable, and is easily reduced to powder. In this experiment it loses 6 per cent. of its weight.

Soluble in mineral acids.

The nitric and muriatic acids totally dissolve the turquois. The solution, in the latter acid, is yellow; and that in the nitric, is colourless.

Nitric solution.

The nitric solution presented the following phænomenon:— 1. with lime-water, a white flaky precipitate—2. by ammonia in excess, a precipitate of the same colour, but more abundant; the supernatant fluid did not acquire any bluish tinge —3. carbonate of ammonia likewise gave a precipitate— 4. with the oxalate of ammonia, the precipitate was very light and very divided—5. precipitate of pot-ash gave a deep blue precipitate.

These preliminary experiments already afford an approximation to a knowledge of the constituent parts of the turquois; they are not sufficient to lead to a regular classification. I therefore chose out of a certain quantity of turquoises, those which

which were the most coloured and the most hard, and I submitted them to the following experiments:

A.—100 parts of turquoises, reduced to powder, were introduced into a small retort: and 300 parts of nitric acid, at 36 degrees, were poured in. After some time, a slight effervescence appeared, which lasted till the solution was complete. The gas being collected in the pneumatic apparatus with mercury, presented all the characters of carbonic acid gas.

Analysis solution in nitric acid carbonic acid escaped.

B.—This nitric solution is white, and of the consistence of syrrup. It was then evaporated to dryness, and the remaining matter made red-hot in a crucible of platina.

C.—The calcination had scarcely changed its colour.

The dissolved matter was a phosphate.

This substance was again dissolved in water, acidulated with nitric acid, with the intention of separating the iron, which might exist in the state of oxide. But the whole was entirely dissolved, which evidently proves that the iron was neither in the state of red oxide nor in that of nitric, but in that of phosphate.

D.—Ammonia in excess was poured on the liquor C, which gave a white precipitate of considerable bulk. This precipitate, after washing and drying, was treated with concentrated liquid pot-ash, which dissolved a certain quantity. The liquor of the non-dissolved portion was afterwards separated from the liquor, and muriate of ammonia added, which separated a white substance, possessing all the properties of alumine. This substance, after the calcination, weighed one part and a half.

Small portion of alumine.

E.—The portion dissolved by the pot-ash was also calcined, and its weight proved to be 82 parts.

F.—Being desirous of ascertaining whether the liquor, from experiment D, did not contain lime in solution, carbonate of ammonia was poured on the fluid, and a precipitate was obtained, which, being slightly dried and heated, was found to be carbonate of lime. Its weight was 8 parts.

Lime.

G.—The supernatant liquor was afterwards evaporated, but it afforded no precipitate; whence it may be concluded, that it contained no magnesia.

H.—Being persuaded beforehand that the precipitate E contained phosphates, it was treated with the sulphuric acid. The matter was afterwards washed, and the waters being put together,

Iron.

together, precipitate of pot-ash was poured on, which formed a precipitate of a fine deep blue, of which the weight, after calcination, was found to be one part and a half. It was red oxide of iron. Care must be taken to heat the liquor, in order to separate the precipitate entirely.

The supernatant liquor held in solution the acid phosphate of lime, which was shewn by the phosphorus it afforded, when treated with charcoal.

A trace of manganese,

I.—This oxide of iron was heated again with a little pure pot-ash. When the whole was in fusion, the matter assumed a deep green colour, and when the cold mass was afterwards dissolved in water, it gave the same colour to the fluid. Upon adding a small quantity of muriatic acid, it became of a fine rose colour. This experiment was repeated on a number of turquoises, and the phænomenon always took place; which evidently shows the presence of a very small quantity of magnesia.

—and manganese.

K.—Being desirous of ascertaining whether the turquois contained phosphate of magnesia, as the experiments of Fourcroy and Vauquelin upon bones, lead to suspect, I treated this substance according to the method indicated by those chemists, in the 47th volume of the *Annales de Chymie*. It was found that 100 parts of the turquois contained two parts of the phosphate of magnesia.

Component parts.

From the preceding experiments, it follows that 100 parts of turquois contain

Phosphate of lime	80
Instead of 82, found in experiment E.	
Deducting the quantity of phosphate of magnesia, before mentioned	0
Carbonate of lime	8
Phosphate of iron	2
of magnesia	2
of manganese, minute quantity	0
Alumine	1
Water and loss	6
	100

Whether all the turquoises be of the same nature as those here examined

Though I obtained similar products in the examination of several turquoises, it cannot yet be decided whether they be indentical. The turquoises used in my experiment are perfectly

fectly similar to those in the Cabinet of Museum of Natural History; and M. Hauy, whom I consulted, could not affirm whether they were truly from Prussia. M. Guyton thinks that there is a difference between the turquoises of Persia and the Occidental.

This philosopher has announced, for several years, in his course of mineralogy, at the Polytechnic School, that the former contained silex. It is possible that turquoises may contain this earth accidentally; but I have not found it in any of those which I examined. This difference ought not, I think, to suspend the classification of this substance by mineralogists. M. Guyton himself has already placed it among fossil bones. This celebrated chemist has likewise made some comparative experiments. He has found that fossil bones assume, in the fire, a colour similar to that of turquoises; that, when digested in water containing pot-ash, they turn blue; and that this blue varies in its shade, by passing from greenish blue to deep blue; and, lastly, that bones, exposed to the air, become white.

Messrs. Fourcroy and Vauquelin have likewise observed, that bones, strongly calcined, often assume a bluish tinge: this colour appeared to them to be owing to the presence of a small quantity of phosphate of iron.

Observation and experiments on iron, as a colouring matter.

There cannot, therefore, any longer exist a doubt respecting the matter which colours the turquoises. If it were necessary to add any thing more to the facts announced, I should observe, that having put the same turquoises which I analyzed, into the hands of Mr. Vauquelin, he did not find a particle of copper in them; and, lastly, I have ascertained that, by pouring into a solution of muriate of lime, phosphate of soda and some drops of muriate of iron at the maximum, the phosphate of lime and of iron is obtained, of which the colour is a greenish blue. We may, likewise, by decomposing the phosphate of soda by muriate of iron at the maximum, obtain a phosphate of iron, which is not white, as some chemists have asserted, but of a green bluish colour.

These reflections, without doubt, are not very important; but I present them as tending to show the possibility of imitating the colour of the turquois, and at the same time to show that iron can, in various circumstances, afford colours similar to those of copper.

VII.

VII.

Account of Mr. CURWEN'S METHOD *of* FEEDING COWS, *during the Winter Season, with a View to provide poor Persons and Children with Milk at that time.*

SIR,

Preface.

EVERY attempt to ameliorate the condition of the labouring classes of the community, is an object not unworthy of public attention; and has, on all occasions, been zealously patronised by the Society of Arts. Under this impression I hope for the indulgence of the Society in calling their attention to an experiment, which I flatter myself will, in its consequence, prove not only highly beneficial to the lower orders of society, but tend likewise to the advancement of agriculture.

Great benefits to be expected from a plentiful supply of milk.

There is not any thing, I humbly conceive, which would conduce more essentially to the comfort and health of the labouring community and their families, than being able to procure, especially in winter, a constant and plentiful supply of good and nutritious milk. Under this conviction, much pains have been taken to induce the landed proprietors to assign ground to their cottagers, to enable them to keep a milch cow. The plan is humane, and highly meritorious; but unfortunately its beneficial influence can reach but a few. Could farmers in general be induced from humanity, or bound by their landlords to furnish milk to those, at least whom they employ, it would be more generally serviceable. Even those who have the comfort of a milch cow, would find this a better and cheaper supply, as they can seldom furnish themselves with milk through the winter. The farmer can keep his milch cows cheaper and better; for, besides having green food, his refuse corn and chaff, of little value, are highly serviceable in feeding milch cows.

General notion that dairies are not profitable in winter.

My object is to combat the prevailing opinion, that dairies in summer are more profitable than in winter. I confidently hope to establish a contrary fact. The experiment I am about to submit to the Society, is to prove, that by adopting a different method of feeding milch cows in winter, to what is in general

general practice, a very ample profit is to be made, equal, i not superior to that made in any other season.

I believe the principle will hold good equally in all situations: my experience is confined to the neighbourhood of a large and populous town.

The price of milk is one-fifth higher in winter than in summer. By wine measure the price is 2*d.* per quart new milk, 1*d.* skimmed.

Local situation of the author.

My local situation afforded me ample means of knowing how greatly the lower orders suffered from being unable to procure a supply of milk; and I am fully persuaded of the correctness of the statement, that the labouring poor lose a number of their children from the want of a food so pre-eminently adapted to their support.

Experiments of a different method of feeding.

Stimulated by the desire of making my farming persuits contribute to the comfort of the public, and of those by whose means my farm has been made productive, I determined to try the experiment of feeding milch cows after a method very different to what was in general practice. I hoped to be enabled thereby to furnish a plentiful supply of good and palatable milk, with a prospect of its affording a fair return of profit, so as to induce others to follow my example.

The supply of milk, during the greatest part of the year, in all the places in which I have any local knowledge, is scanty and precarious, and rather a matter of favour than of open traffic.

Provision.

Consonant with the views I entertained of feeding milch cows, I made a provision of cabbages, common and Swedish turnips, kholrabi, and cole seed. I made use also of chaff, boiled and mixed with refuse grain and oil cake. I used straw instead of hay for their fodder at night.

The greatest difficulty, which I have had to contend with, has been to prevent any decayed leaves being given. The ball only of the turnip was used. When these precautions were attended to, the milk and butter have been excellent.

Having had no previous knowledge of the management of a dairy, my first experiment was not conducted with that frugality requisite to produce much profit.

The first experiment.

I sold the first season, between October 1804, and the 10th of May 1805, upwards of 20,000 quarts of new milk. Though my return was not great, I felt a thorough conviction that it proceeded

proceeded from errors in the conduct of the undertaking; and that, under more judicious management, it would not fail of making an ample return, which the subsequent experiment will prove. In the mean time I had the satisfaction of knowing that it had contributed essentially to the comfort of numbers.

In Oct. 1805, my dairy recommenced with a stock of 30 milch cows; a large proportion of these were heifers; and in general the stock was not well selected for giving milk; for they were purchased with a view of their being again sold as soon as the green crop should be exhausted. If the plan be found to answer under such unfavorable circumstances, what may not more experienced farmers expect?

By the end of this present month, I shall have sold upwards of 40,000 quarts of milk

The quantity of food, and its cost, are as follow. The produce of milk from each cow upon 200 days, the period of the experiment, is calculated at no more than 6 wine quarts in the 24 hours: this is to allow for the risk and failure in milk of some of the heifers. A good stock, I have no doubt, would exceed 8 quarts in the two meals, which would add 100*l.* to the profit.

Daily cost of feeding one milch cow:

Accounts, statements of the feed and produce.

	£.	s.	d.
Two stone of green food (supposing 30 tons of green crop on an acre, at ¼*d.* per stone would pay 5*l.* per acre) at ¼*d.* per stone of 14*lb.*	0	0	0½
Two stone of chaff boiled, at 1*d.* per stone	0	0	2
Two *lbs.* of oil cake at 1*d.* per *lb.* costing from 8*l.* to 9*l.* per ton	0	0	2
Eight *lbs.* of straw at 2*d.* per stone	0	0	1
	0	0	5½

The chaff, beyond the expense of boiling, may be considered as entirely profit to the farmer; 2*d.* per stone for straw, likewise leaves a great profit. Turnips also pay the farmer very well at ¼*d.* per stone.

Expense of feeding one milch cow for 200 days, the period upon which the experiment is made:

200 days

	£.	s.	d.
200 days keep of one milch cow, at the rate of 5½d. per day	4	11	8
Attendance	2	0	0
Supposed loss on re-sale	2	0	0
	£.8	11	8

Return made of one milch Cow in 200 days milking:

	£.	s.	d.
6 quarts per day, at 2*d.* per quart, for 200 days	10	0	0
Calf	2	0	0
Profit on 20 carts of manure, 1*s.* 6*d.* each	1	10	0
	13	10	0
Clear gain upon each milch cow	£.4	18	4

This gives a profit upon the whole stock of £.147 10*s.* The profit of another month may be added before a supply of milk can be had from grass, which will make the balance of profit 167*l.* 18*s.* 4*d.* This profit, though not as large as it ought to have been, had the stock been favourable for the experiment, far exceeds what could be made of the same quantity of food by fattening cattle. Were the two quarts to be added which on a moderate computation might be expected, the gain would then be £.267 16*s.* 4*d.* The trifling quantity of land from which the cattle were supported, is a most importanf consideration. One half of their food is applicable to no other purpose, and is equally employed in carrying on the system of a corn farm. I have found oil cake of the utmost advantage to my dairy, promoting milk, and contributing greatly to keep the milch cows in condition. The best method of using it, is to grind it to a powder, and to mix it in layers and boil it with the chaff: half the quantity in this way answers better than as much more given in the cake, besides the saving of 2*d.* a day on each beast. This I was not aware of on my first trial. The oil cake adds considerably to the quantity and richness of the milk without affecting its flavour. The refuse corn was likewise ground and boiled: it is charged also at 1*d.* per pound. I

Observation on the considerable profit obtained.

make use of inferior barly to great advantage. A change of food is much to the advantage of the deary. Potatoes steamed would answer admirably; but near towns they are too expensive.

The milk was better in quality than usual.

By repeated trials it was found that 7 quarts of strippings, wine measure, gave a pound of butter, while 8 quarts of a mixture of the whole milk was required to produce the same weight. Contrast this with milk produced from the feeding of grains, 20 quarts of which will scarce afford a pound of butter.

Produce of butter usually had from milk.

The Agricultural Report of Lancashire, treating on the milk in the neighbourhood of Liverpool and Manchester, states 18 quarts with a hand churn, and 14 or 15 with a horse churn. In a paper published by the Bath Society, 12 quarts are said to give a pound of butter: but wether ale or wine measure, is no specified. A friend of mine, who feeds his milch cows principally on hay, finds 16 wine quarts will not yield more than 17 ounces of butter, and this upon repeated trials.

The milch cows, treated according to my new plan, have been in excellent order both seasons, and are allowed to be superior to any in the neighbourhood.

Cole seed I have found to be the most profitable of all green crops for milk; and it possesses the further advantage of standing till other green food is ready to supply its place.

Comparative statement.

To ascertain the benefit and utility of a supply of milk, both to the consumer and the public will be best done by comparison.

To prove this, let us contrast the price of milk with other articles of prime necessity, and consider how far it affords a greater produce from a less consumption of food.

I cannot here omit observing, at a moment when Great Britain can hope for no further supply of grain from the continent, and must look for and depend on her own resources for feeding her population, every mean by which the quantity of victuals can be augmented, is an object of great public concern.

General numirical deduction.

Each milch cow, yielding 6 quarts of milk per day, furnishes, in the period of 200 days, 2,400 pounds of milk, or 171 stone of 14 pounds. equal to twice her weight, supposing her in a state fit for killing, with a third less food, and at one half less expense. The milk costs £.10; whilst the same weight of butchers meat at 6*d.* per pound would amount to £.60.

Taking

Taking the scale of comparison with bread, we shall find a Winchester bushel of wheat of the usual weight of 4 stone and 4½ *lb.* when manufactured into flour of three sorts yields :

Of first flour -	2 st.	9 lb.
Of second -	0	7 lb.
Of third -	0	7 lb.
	3	9 lb.
Lost by bran, &c. -	0	9½lb.

The present cost is 10*s.* 3*d.* 2,400*lb.* of the three sorts of flour will cost £.23 3*s.* 9*d.* To make it into bread, allow 1*s.* per bushel, which makes the cost of bread £.26 10*s.* 9*d.* or something more than 2½*d.* per *lb.* exceeding twice the price of the same weight of milk. To furnish 2,400*lbs.* of bread, requires 47 bushels, or the average produce of two acres of wheat.

Three acres of green food supplied 30 milch cows with 2 stone each of green food for 200 days. Two stone of hay each for the same period would have required 75 acres of hay. Chaff can scarcely be considered as of any value beyond the manure it would make, which shows the profit of keeping milch cows in all corn farms.

Certificates of the quantities of milk sold and money received accompany this.

If the Society of Arts, &c. think the experiment worthy their notice and approbation, I shall be highly flattered. At all events, I trust they will accept it as a small tribute of respect and gratitude for the many favours conferred upon their

Obedient and very humble Servant,

J. C. CURWEN.

Workington Hall, April 18, 1806.
To Dr. C. TAYLOR, Secretary.

VIII.

Some Account of the Manufacture of Forged Iron Vessels, at Fromont. By M. Ch. Hersart.*

Fabrication of vessels of forged iron, for culinary and other uses.

THE operations of forging vessels of cast iron may be divided into three distinct parts: 1st, the method of forging the plates; 2d, that of forging the cake or parcel; 3rd, the cold hammering Of those we shall speak in the order here mentioned, which is likewise the order of fabrication.

To Forge the Plates.

The iron for this manufactory must be very soft and malleable. It has usually the form of bars, ten or twelve feet long; each bar having the form of a long truncated square pyramid. This form is necessary in order to obtain plates of different diameter. The small base is a square of ten lines, or twelfth of an inch, and the greater eighteen lines.

The assistant puts one of these bars in the fire, and when the heated part is ignited, the master forgeman carries it to the small tilting hammer, which is not different from those used in drawing out steel bars. He places the bar on the anvil, not upon one of its faces, but on an edge, as, in this position, the iron is less subject to crack. According to the size of the plate intended to be hammered out, the workman strikes a greater or less portion of the bar, presenting it in all situations to the hammer, in order that the plate may obtain a circular form. Between the plate and the bar itself, he fashions a small neck to facilitate its separation. In this manner, the workman continues to forge the plate on both its faces as long as the heat allows, after which he carries the bar to the anvil, and applies a cold chissel to the neck, upon which his assistant strikes in order to separate the plate from the bar. This last is then returned to the fire, in order to continue the operation in making a second plate. Sometimes, but this is only when the plates are small, the workmen make three at once.

When a sufficient number of plates has been thus fabricated, as they are of different sizes, namely, from three or four inches diameter

* Journal des Mines, No. 112.

diameter to a foot, the workman disposes them in parcels, of which each contains four of equal dimensions, and then carries one of them to the hearth of the furnace, where the assistant takes them in the large tongs, Fig. 1, Pl. VI, and puts them into the fire, taking care to change their position often; and when the brass is red hot, the master workman, who holds a small pair of tongs in each hand, carries it under the tilting hammer, after having spread charcoal powder between the plates, to prevent their welding together. The two pair of small tongs have the form of Fig. 2, and are used to give a circular motion to the parcel, and to keep it on the anvil. When he has finished hammering it, he changes the order of the four plates, and in making this change, he is careful to take notice whether any of them have cracked; and where he perceives any crack, he applies the cold chizel, or a wedge to the place on which the assistant gives a blow.

Fabrication of vessels of forged iron for culinary and other uses.

After having changed the situation of the plates in such a manner that the two outside plates become the interior ones, he places this parcel on the hearth, and takes another set, which the assistant has caused to be heated, and he subjects this to the same operation of the hammer. In this manner the process is conducted until the required dimensions are obtained, namely, after five or six heatings. He then places the plates on the ground to cool; and when cold, he cuts them circularly one at a time, with the large hand-shears, Fig. 3.

This being done, each face of the plate is severally covered with a mixture, formed of the oxide of lead and oxide of tin, pulverized and mixed with a little water; or, instead of this mixture, clay, diluted in water, may be used, as I have seen practised. Either of these will prevent the plates from welding together, and for that purpose it is that they were applied.

Forging the Cake.

The workman takes seven plates of the same size, coated as before described, with the oxide of lead and tin, and he places them upon each other. These seven being placed on two others of larger size, constitute what is called a cake, which is put into the fire by means of large tongs, not differing from the former, except in the mouth, or claws, which are rather higher and curved, as is seen in Fig. 4.

Fabrication of vessels of forged iron for culinary and other uses.

When the cake is red hot, the assistant, who always has the management of the fire, takes it to the edge of the furnace, where the master workman bends the two large plates in one part, and takes up the cake with the tongs already mentioned, Fig. 2, when he carries it to anvil of the small forge hammer, in order to bend the edge of the two great plates entirely round. The difference between the diameter of the great and small plates, is about two inches: when this is done, he puts the cake again into the fire; and when red hot, he carries to a smaller caking hammer and that used before, but fixed and moved in the same manner. The anvil is a rectangular parallepipedon, which rises above the ground not more than one foot; and it has three pieces of iron bended to a right angle, at the height of the angle, which affords three branches converging towards the anvil, and serving to facilitate the operation of moving the cake during the work next to be described. See the plan and elevation traced, Fig. 10.

The workman being seated before his hammer, takes the cake with two small pair of tongs, and gives it a continual circular motion: during this commencement of the work, he hammers it only on the edge, after which he ignites it, he again carries it to the same hammer, first wetting the edge of the plates to diminish the heat which would only incommode him. By this second forging, he carries his stroke nearer to the center, still continuing the circular motion. By repeating the same operation as far as for eight times, continually approaching the center, the edge rises every time, and the assemblage of plates become more and more hollow. Accordingly, as this figure encreases, he finds it necessary to change his tongs for others, which differs from the first in the elevation of one of the jaws, and the extremity of the handle, Fig 5. After seven or eight ignitions, he carries the cake to a kind of anvil, the form of a figure 6, where he holds it with small tongs, Fig. 7, in order to complete the sides, which is done by the workmen hammering in succession, the hammer of the assistant being heavy and double-handed, when this is upon two at once. It is speedily done, and followed by another nearly similar on the bottom of the vessel, by a second hammer, placednear the first, striking on a kind of square anvil. Young girls, afterwards, are employed in scraping the bottom with an iron rod, Fig. 9. One foot and

and a half in length, terminating at one of its ends in a flattened small termination of steel. After this is done, the workman takes three vessels, one after the other, and presents them under a third hammer, placed near the two first, and moved like them by the same arbor, which carries a small tripping wheel, moved by water. The vessel is placed on the anvil, so that the hammer, which is pointed at its striking extremity, enters into its cavity. The workman holds the vessel, and shifts its position with his hands and knees. Every stroke of the hammer leaves a slight cavity of the size of a pea, which forms different designs, according to the motion which the workman gives to the vessel. These outlines are not made for the sake of beauty, but to give strength and firmness to the vessel by hammer hardening it. The young girls, afterwards, take the vessels and scrape the interior sides, as was done with the bottom; and lastly, the workmen, on two kinds of anvils, the one plain and circular for the bottom, and the other semi cylindric for the sides, completes their figure with a wooden mallet. Small cracks sometimes appear in the vessel, which the workman close, and the matter is suffered to cool; after which, the cake, which now has the form of a tuencated cone, is carried against a piece of iron bended two ways, Fig. 8, and drove into the wooden block, which supports the gudgeon of the arbor of the hammers. This doubly recurved iron serves to retain the cake which enters under it, and by that means allows the small tongs, Fig. 7, to raise up the edges of the two great plates, which, in part, covered the seven small ones. This being done, the vessels, or hammered pieces, are taken out from within each other. The first is always perforated on account of the immediate purchase of the hammer, and that of the air, which, partly converted into scales, fall out by the immediate action of the hammer. As these vessels, when taken out, are more or less bended, the assistant sets them to right by a few strokes of the hammer, after which the master workman cuts their edges with the shears.

Fabrication of vessels of forged iron for culinary and other uses.

Cold Hammering and finishing.

After the vessels are cut round, they are delivered to another workman, who takes them to his separate shop to finish. His first operation is to set the conical surface fair by means of a small

a small hammer, upon a proper tool. The workman holds the vessel with his right hand with his small tongs, 7; and with his left hand, without tongs, taking care to turn it round continually. Sometimes he performs this operation with a stroke of the hammer; and the complete finish is made by cutting the edges with scissars, similar to those before described.

The furnace made use of is a simple forge furnace, and the fuel is charcoal of fir, excited by wooden bellows.

IX.

Description of an Astronomical Circle, and some Remarks on the Construction of Circular Instruments. By JOHN POND, *Esq.* *

From the Philos. Transactions, 1806.

Astronomical circle of two feet in diameter,

THE observations which accompany this paper were made at Westbury in Somersetshire, in the years 1800 and 1801, with an astronomical circle of two feet and a half diameter, constructed by Mr. Throughton, and considered by him as one of the best divided instruments he had ever made; a drawing of it, with a short description, is anexed to the observations. (Plate VII.)

—applied to determine the declinations of some principal stars.

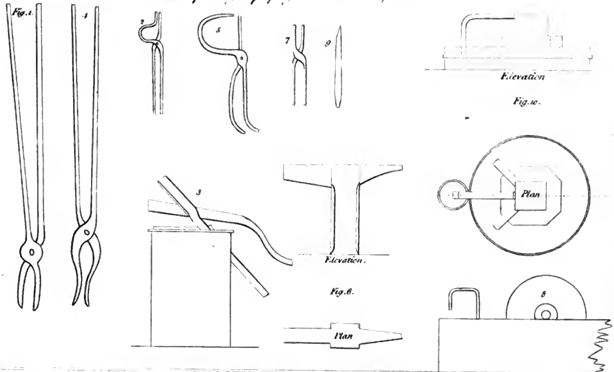
When this insrtument came into my possession, I thought I could not employ it in a more advantageous manner, than in endeavouring to determine the declinations of some of the principal fixed stars†. The various catalogues differed so much from each other, and such doubt existed as to the accuracy of those which were thought most perfect, that the declinations of few stars could be considered as sufficiently well ascertained for the more accurated purposes of astronomy.

The

* The title of this Paper, in the Transactions, is, "On the Declinations of the principal fixed stars, with a description," &c but on account of the extent of the tables of observations, I must refer the astronomical reader to the Transactions for them. The number of stars, observed at Westbury, were 29; and of those compared with the Greenwich observations, 37.—The plate could not be finished till the next Number.

† At that time Dr. Maskelyne's late Catalogue was not published.

Manufactory of forged Iron Vessels.



The advantages that have resulted from the excellent method persued at Greenwich, of observing constantly the transits of a few stars, to obtain accurately their right ascensions, induced me to follow the same method for determining their declinations; and for a considerable period I constantly observed them on the meridian, whenever they passed at a convenient hour; usually reversing the instrument in azimuth at the end of every day's observation; never considering any observation as complete that had not its corresponding one in a short interval of time. When this circumstance is not attended to, I think, a great part of the advantage arising from the circular construction is lost. Method of observing, &c.

The observations themselves will show, if they have been made with the requisite care and attention to merit the notice of astronomers; for it is one of the many advantages of circular instruments, that, from the observations made with them, we may infer with great precision not only the mean probable error, but likewise the greatest possible error to which they are liable. From a careful comparison of the errors of collimation, as deduced from different stars, I concluded that the greatest possible error was 2",5, and the mean error about 1"; and by a comparison with other observations with similar instruments, it will be seen that this supposition was well founded, since nearly the same quantities are deduced by another method to be considered hereafter. Accuracy of the observations. Greatest error of collimation.

The polar distances are annexed to each observation: a method which I borrowed from Mr. Wollaston, and which is rendered veryeasy by employing his useful tables calculated for that purpose. This practice of reducing every day's observations cannot be too much recommended, as the labour of calculating accumulated observations is thus rendered unnecessary. Polar distances

When I had deduced the declinations of these stars from my own observations, continued long enough to divest them of all error, except that arising from defect in the divisions of the instrument. I was desirous of comparing them with the observations made by the others; and I have subjoined a comparison of them with all those which I could procure, that seemed entitled to confidence. In the first column are the observations made at Greenwich, as published in 1802 by the Astronomer Royal; the second column, contains a catalogue Comparison with other observations. —namely, at Greenwich, at Armagh, at Palermo, and

by the author at Westbury. logue deduced from some observations made at Armagh with a very large equatorial instrument, constructed by Mr. Troughton; in the third column, are the observations of Mr. Piazzi, of Palermo; and in the fourth, those made at Westbury. All the above mentioned observations are arranged in the order of their polar distances, and the positive deviations separated from the negative; that the cause of error in any of the instruments, may be the more easily detected, as likewise any mistake in the assumed latitudes of the respective places of observation.

General catalogue. A general catalogue is then added; which is deduced, by taking the mean, generally of the above four; but in some places, a few detached observations that I have accidentally procured of other circular instruments, have been included. The utility of this investigation is not merely confined to the determination of the polar distances of the stars; as, besides this, some valuable information on other points may be obtained. In the first place, upon examining the variations that appear in these observations a question naturally occurs, whether, by changing the assumed latitudes of the respective places of observation, a nearer coincidence might not be obtained? And I find, that to make the positive deviations equal to the negative, the following corrections should be applied to the colatitudes;

Greenwich	$+ 1''$
Armagh	$+ 1'',3$
Palermo	$- 1''$
Westbury	$- 0'',25$.

New correction of the latitudes from the deviations of observations This method of correcting the latitudes has, I believe, never been employed; but it seems reasonable to suppose, that, when thus corrected, they will be nearer the truth, than those determined by the usual method: for the same reason, that the declinations of the stars resulting from a general comparison, are more likely to be accurate, than if deduced from any one single set of observations; but if the Greenwich instrument should be affected with any errors independant of the divisions, in that case, we should be unable to infer any thing decisive, as to the latitude, by the above method. But from a comparison of the observations of γ Draconis, observed at Greenwich and Westbury, the latitude of Westbury being previously corrected by the above method, I am inclined to believe the latitude of Greenwich requires a very small correction, certainly

certainly not exceeding a second. The result I obtain by a very careful investigation by methods, entirely independent of the Greenwich quadrant, is 51°.28′.39″,4.

The comparison of observ.

I consider this comparison as interesting likewise on another account; it is an object deserving of curiosity to examine the present state of our best astronomical instruments, and to ascertain what may reasonably be expected from them. The superiority of circular instruments is, I believe, too universally admitted, to render it probable that quadrants will ever again be substituted in their place. But the Greenwich quadrand is so intimately connected wih the history of astronomy, the observations that have been made with it, and the deductions from those observations, are of such infinite importance to the science, that every circumstance relating to it cannot fail of being interesting Now, when it is considered that this instrument has been in constant use for upwards of half a century, and that the center error, from constant friction, would, during this time, have a regular tendency to increase, it will not appear at all surprising, if the former accuracy of this instrument should be somewhat impaired. With a view, therefore, of ascertaining more correctly the present state of an instrument on which so much depends, I have exhibited in one view the polar distances as determined by circular instruments alone; the respective co-latitudes being previously corrected by the method above mentioned, and I have compared the mean result with the Greenwich Catalogue, that the nature and amount of the deviations may be seen, and if it be judged necessary, corrected. I should add, that by some observations of the sun at the winter solstice in 1800, the difference between the Greenwich quadrant and the circle was 10 or 12″, the quadrant still giving the zenith distance too little.

shows the present state of instruments.

General Description of the Instrument.

Description of the author's circular instr. made by Troughton.

The anexed plate represents the circle in its vertical position. It was originally made to be used likewise as an equatorial instrument, a circumstance I need not have mentionned, but as an apology for slightness of its construction, which the artist, who made it, would not have recommended, had the instrument been intended for the vertical position only.

The

Decl. circle.

The declination circle, 30 inches in diameter, is composed of two complete circles; the conical radii of which are inserted at their bases in an axis about twelve inches long, leaving sufficient space between the limbs for a telescope $3\frac{1}{2}$ feet long, and an aperture of $2\frac{1}{4}$ inches, to pass between. The two circles are firmly united at their extreme borders by a great number of bars, which stand perpendicular between them; the whole of which will be readily understood by referring to the figure. The square frames, which appear as inscribed in the circle, were added to give additional firmness to the whole.

The circle is divided by fine lines into 5′ of a degree; and subdivided into single seconds by two micrometer microscopes, the principles and properties of which are now too well known to require any particular explanation.

Great improvement that the microscopes are capable of having their position varied by a circular motion of the diameter which connects them.

At the time these observations were made, the microscropes were firmly fixed opposite to the horizontal diameter; but when I considered that, by continuing the observations, the error of division would never be diminished, I suggested to Mr. Troughton the possibility of giving a circular motion to the microscopes; though I confess with very little hope, that the thing was really practicable in an instrument previously constructed on other principles. Mr. Troughton approved of the idea, and executed it in a very ingenious manner. His talents, as an artist, are too well known and too highly appreciated, to stand in need of any praise from me; yet I should consider myself as deficient in justice, if I did not endeavour to call the attention of the reader to the skill and ingenuity, which have been employed not only in this very important alteration, but in every contrivance that is peculiar to the instrument, which is the object of our present consideration.

These microscopes can now revolve about 60° from their horizontal position; and it is easy to comprehend, that, by this valuable improvement, all errors of division may be completely done away, without any of the manifest inconveniences of the French circle of repetition; which, though a very ingenious instrument, and admirably adapted to some particular operations, will, I think, never be adopted for general use in our observatories.

The plumb-line

The plumb-line, a very material part of this instrument, is suspended from a small hook at the top of the tube at the left hand

hand of the figure. It passes through an angle, in which it rests in the same manner as the pivot of a transit instrument does on its support. At the lower end of the tube which protects it, a smaller tube is fixed at right angles, which contains microscopic glasses so contrived, that the image of a luminous point, like the disc of a planet, is formed on the plumb-line and bisected by it. Great attention should be given to the accurate bisection of this transparent point by the plumb-line *at the moment of observation.* It is absolutely essential in instruments of this construction, to consider the observation, as consisting in two bisections *at the same time*: the one of the star by the micrometer, the other of the plumb-line-point by the plumb-line. The least negligence in either of these bisections will render the observation unsuccessful.

Upright pillars and vertical axis, passing down in stone pedestal.

The two strong pillars, which support the axis of the vertical circle, are firmly united at their bases to a cross bar; to which also the long vertical axis is affixed, and which may be considered as forming one piece with them. The stone pedestal is hollow, and contains a brass conical socket, firmly fastened to the stone, and reaching almost to the ground. This socket receives the vertical axis, and supports the whole weight of the moveable part of the instrument, which revolves on an obtuse point of the bottom; the upper part of this vertical axis is kept steady in a right angle, having two springs opposite the points of contact, which press it against its bearings, and it thus turns in these four points of contact with a very pleasant and steady motion.

Adjustment of the vertical axis.

The bar, in which the vertical axis is thus centered, is acted on by two adjusting screws in directions at right angles, and perfectly independent of each other. By these motions, the axis may be set as truly perpendicular, as by the usual method of the tripod with feet screws, which could not in this case have been employed.

The frame to which this apparatus is attached, is fixed to the corners of the hexagonal stone, by the conical tubes; between which and the stone, the azimuth circle (which forms one piece with the vertical axis) turns freely. The azimuth circle of two feet diameter, consists of eight conical tubes, inserted in the vertical axis, and which are united at their ends by the circular limb; this is divided and read off exactly in a similar manner to the other circle.

A level

—and of the horizontal axis. A level remains constantly suspended on the horizontal axis, which is verified in the same manner as in a transit instrument. There are forcing screws for this purpose, which pass through the bar on which the vertical columns stand, and these by pressing against the long axis, produce a small change in the inclination of the upper part of the instrument, without altering the position of the azimuth circle or its axis.

The application of the plumb-line, as already described, is peculiar to the instruments made by Mr. Troughton: it regards the vertical axis rather than any other part, and is, in fact, exactly analogous to the usual verification of a zenith sector.

On the adjustment of circular instruments. During the period in which I was engaged in making observation with circular instruments, I was led to consider the advantages and inconveniences of the usual method of adjusting them; and it appeared to me, that the essential part of their construction, which relates to their adjustment, was capable of being improved.

In order to render the nature of the improvement, which I wish to propose, more intelligible, I ought previously to remark, that there are, at present in use, two modes of adjusting these instruments, which are founded on different principles.

Two methods now in use. Two points on the circle are brought into a vertical line by means of the plumb line, and the index is then brought to zero. In the one, two points are taken on the limb of the circle; and when these are brought into a given position, by means of a plumb-line passing over them, the microscope or index is made to coincide with the zero point of the divisions: by this method, the error in collimation remains constant; and if the adjustment is by any accident deranged, it can easily be rectified, and there will be no absolute necessity for frequently reversing the instrument: so that this method seems well adapted for large instruments, particularly if placed on stone piers. But it is liable to this defect, that the adjustment cannot be examined at the moment of observation; and if any change should take place in the general position of the frame work, the observation will be erroneous without the means of detection. It was probably to avoid this inconvenience, that Mr. Troughton, in most of his instruments, particularly if they were intended to move freely in azimuth, has preferred the other method.

Objection. The frame work may vary, and render the observation erroneous.

Or. 2. The plumb-line being attached to the support of In this case, the plumb-line is attached to one of the pillars, which support the microscopes in the way above described; and it has no reference to any fixed points or divisions on the limb of

of the circle, but only insures a similarity of position in the index, for each position of the instrument; and, provided that the plumb-lime apparatus was free from all danger of derangement, this would be sufficient. This verification may be rendered perhaps more intelligible, by considering that a circular instrument, in whatever manner its vertical axis be placed, indicates by a double observation, the angle which the object makes with the axis, round which the whole instrument has revolved in passing from one position to the other. For let Pp be the axis, Tx the telescope x in one position; it is evident, that in turning the instrument half round, ty will then be the position t of the telescope; Px being equal to Py. The arc xy, which the telescope passes through to regain its former position, is the quantity really given by the instrument; and if the axis Pp be vertical, half this quantity is the true zenith distance of the object. Now the intention of Mr. Troughton's verification is to insure a vertical position to the axis Pp.

the microscopes, insures their position, and each observation is made with a corresponding one, after reversing the instrumt. in azimuth.

For instruments which rest on moveable pillars, and turn freely in azimuth, this method is much to be preferred; but it is not without a considerable defect: for, if by any derangement in the plumb-line apparatus, the error in collimation be changed, it cannot be restored with certainty to its former position; so that sometimes a very valuable series of observations may be lost, for want of a corresponding one to compare with it. The mode which I propose to adopt to remedy these inconveniences, will enable us to combine all the advantages of the two methods above described: it is extremely simple in its principle, and easy of execution, for it merely consists in uniting on the same plumb-line the two principles already explained.

Objection, that the corresponding observation cannot be had if the plumb-line should be deranged.

Two very fine holes should be made in the farther limb of the circle, and two lenses firmly fixed opposite to them, in the other, which should each form an optical image of its corresponding dot or hole, in the tube through which the plumb-line passes.* It will be best, if these dots are made exactly

Union of both methods in one by the author.

* As these transparent dots are intended to be bisected by the plumb-line, they must be capable of the necessary adjustments, both for distinct vision, and for placing them in an exact diameter.

It may be found more convenient in practice to arrange the whole apparatus in sliding tubes, but in whatever way the contrivance be executed, the points should ultimately be fixed as firmly as the divisions of the instruments.

exactly in a diameter, as they may then be used in two positions. Beneath these should be formed the image of a luminous point, according to Mr. Troughton's present method, by an apparatus attached to the plumb-line tube; when the two points on the circle move away, by the necessary operation in observing, the lower point will remain stationary, and indicate any change of position in the whole instrument, if such should accidentally take place, and which by the other method alone would have passed unnoticed.

The second is most deserving of confidence.

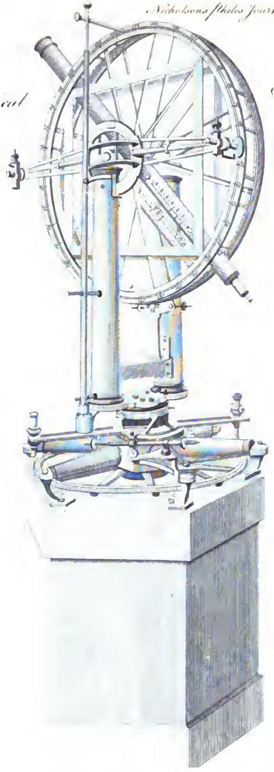
The contrivance above described was executed for me at my request by Mr. Troughton, and is represented in the plate; but by some accident a part of the apparatus was broken in putting it together, so that I never was able to use it. As each apparatus for this adjustement is quite independent of the other, no possible inconvenience can attend their application, as either may be employed alone, at the option of the observer. But as any verification requiring many bisections is objectionable, I would in general certainly prefer Mr. Troughton's method, and only have recourse to the other, when there was reason to suspect that some alteration had taken place to render it necessary.

Errors of division in the instrument,

One more circumstance respecting the instrument remains to be noticed: when the divisions were first examined by opposite readings, 1",25 was the greatest possible error which was to be apprehended, and 0",7 the mean error; but in its journey it seemed to have suffered some very small derangement in its form: this was discernible both from examining the opposite readings; and by deducing the error of collimation by zenith stars, and comparing it with that found by an horizontal object, there was constantly perceived a difference of 3" between the error of collimation deduced from γ Draconis and by an horizontal object; and this quantity was very uniformly distributed through the intermediate arc. In what particular manner the observations would be affected by this derangement I willl not venture to decide, but I think it most likely that it has only rendered the instrument rather less accurate than it was originally, as is above stated. I have before observed the great advantage the circle possesses of showing the amount of its own errors. These may be determined with great certainty by examining the errors of collimation as deduced

--determinable with great certainty from the stars.

Astronomical Circle.



ced from different stars. This method is founded upon the supposition that half the difference of the two extreme quantities is the greatest error of division, which has in this case influenced each result in an opposite direction. For instance, let us suppose the errors of division never to exceed 2", but occasionally to amount to that quantity, on several parts of the circle; it will then sometimes occur that each index will give 2" too much in one position of the instrument, and 2" too little in the other; there will then appear a difference of 4" in the error of collimation; but the observations in these extreme cases will not on that account be the less to be depended on; on the contrary, the probability is in favour of their superior accuracy.

Nor, on the other hand, will those observations which give the mean error of collimation deserve greater confidence than the rest, since it is evident that some of them may be, and most probably are, affected with the greatest possible error; for we suppose the most erroneous observation to arise from the greatest error of division occurring on each of the four arcs in the same sense, that is all *plus* or all *minus*; nevertheless, the observation thus erroneous, will give the mean error of collimation.

The great accuracy of observing by circular instrument is to be expected from the revolving microscopes.

By an attentive consideration of these circumstances, corrections might perhaps be obtained which would somewhat diminish the probability of error. But it is to the principle of the revolving microscopes, that in the future construction of instruments we should look for perfection. In the French circle of repetition, too great a sacrifice is made to the supposed advantage of reading off a great number of observations at once. Our best instruments are too well constructed to stand in need of this contrivance, as the divisions on a two-feet circle are read off with precision to a single second. The errors of simple division alone are those which continued observations have no tendency to diminish; these, by making the microscopes revolve, may be completely done away. An instrument thus constructed would be well adapted to detect small motions in the fixed stars which hitherto have escaped notice, or such as are but imperfectly known, for we cannot reasonably conclude that what is termed the proper motion of a star, is so uniform and constant, that being once determined, it will remain always the same.

The method of repetition does not correct the errors of division.

X.

On the Utility of the Lichen of Iceland as Food. By Professor PROUST. *Abridged from a Memoir in the Journal de Physique for August,* 1806.

Enquiry whether the lichen may not afford a resource in times of famine.

THE Professor begins his memoir by remarking, that the severe pressure of famine, and the diseases which follow in its train, were such as, a few years ago, directed the attention of every thinking man to the means of affording subsistence to the poor. And, under this interesting head of enquiry, he asks, whether the lichens, of which numberless species cover the rocks throughout Spain, and which constitute a large part of the food of the Laplander and people of Iceland, do not promise advantages well deserving investigation.

The Iceland lichen grows in Spain, and elsewhere in southern Europe.

Don Mariano La Gasca has discovered, in the environs of the monastery of Harvas, the very lichen of which the Icelanders prepare a food, which travellers affirm to be as substantial as wheaten bread. This monastery is situated at a considerable elevation in the mountains which separate the province of Leon from Asturias. It is also found in great abundance in many parts of the latter province. It was before known to grow in many places of Europe, but has been principally spoken of as an article of medicine; concerning which our author speaks in a general way, and without any marks of approbation, in the course of two pages, through which it is needless to follow him.

It is of little value for dyeing.

When Mr. Proust received the lichen from La Gasca, he found reason to examine it rather as an article of food than a dyeing drug; being induced to do so by the reports of travellers, collected in the Apparatus Medicaminum of Murray, which are the following:

Reports of travellers concerning its use as food in the north.

Von Troil informs us that the Icelanders made excursions of a week or a fortnight to the districts which produce the Lichen, which they carry home and keep in sacks till the time of use, when they wash it and reduce it to flour. Olafer asserts that two measures of this powder are as nourishing as one measure of wheat flour. After soaking it in water for a day to take away its bitterness, they boil it with whey till it is

is converted into jelly. They eat it either hot or cold, with an additional portion of whey or milk.

According to Benzelius, the Laplanders boil the lichen in one or two waters and throw away the decoction. They afterwards wash it in cold water and boil it in milk, after having crushed it. This soup is seasoned for use with salt. The author shows that a part of the nutriment is thrown away along with the decoction.

Some Swedish botanists, who travelled in Lapland during the summer of 1788, when the north of Germany and the west of Bothnia, were afflicted with a cruel famine, subsisted upon it for a fortnight. They soaked it all night in hot water, and in the morning they boiled it with milk.

It is very nourishing to animals.

Scopoli informs us that, in Carniola, there is no food known which fattens animals so speedily as this lichen. Lean horses and oxen are taken to the places where it abounds, and in less than four weeks they become very hearty and fat.

According to Pallas, the people of the northern part of Asiatic Nessia support themselves upon a lichen when their other provisions fall short.

After these introductory particulars our author proceeds to give the result of his observations and experiments.

Method of preparing it previous to washing.

The lichen is cleaned by picking out the mosses and fragments of wood, and washing away any earth which may lie among its roots, by rubbing it with the hands under water. A very short time of immersion in cold water restores the colour and humidity of this vegetable, and more than doubles its weight. In order that the water may extract its bitterness, it is necessary to crush or divide its parts by cutting or pounding. In this state water extracts, in the course of three hours, a bitter and slightly yellow juice, not absolutely disagreeable, but very supportable when the plant is prepared by simple boiling, without maceration. The extraction of the bitter principle by cold water diminishes the weight about three parts in the hundred. Hot water takes out the bitterness more speedily, but at the same time extracts about an equal quantity of the nutriment. But this trifling loss is compensated by the speed with which the effect is produced. The bitter principle is an extractive matter, which strikes a brown with iron, and is used for that purpose by the Icelanders. It is

Cold water takes away its bitterness,

is not, however, so good as to be preferable in the European dye-house.

but not its nutritive principles.

Such infusions as are here mentioned would extract the most useful parts from vegetables in general, particularly gums and sugars; but the lichen sustains no considerable loss, because its nourishing and soluble parts are very different from sugar, gum, or even farina.

It requires a slight boiling, and gains twice its weight.

One quarter of an hours boiling in water is sufficient to cook the lichen, and render it as tender for use as can be wished; at the same time that it extracts the soluble principle. After the boiling, one pound of the lichen from its dry state, three pounds of the plant, fit to be served up as a vegetable in the solid state, are obtained. It is remarkable that the boiled lichen, when pressed in a cloth, to expel the superfluous water, recovers its first volume as readily as a sponge. So far from resembling those vegetables which have a ligneous structure, which requires considerable boiling, it has the elasticity of some champinions, and eats like the very tender cartillages of animals. The lichen has not, however, any analogy to animal substances.

When boiled and dried scalding water instantly swells it out, and renders it eatable.

The boiled lichen when dry, and preserved in that state, resumes its elasticity in an instant, and becomes fit for the table by pouring boiling water upon it. Fresh water, or sea water, are equally applicable to this object. Some persons, who have eaten it at the author's table, and knew by experience what an invaluable resource fresh vegetables are in long voyages, remarked, that a provision of boiled lichen would afford a fresh sallad, no less agreeable than advantageous to the health, under circumstances of this nature. The author thinks that this sallad, which, without any consumption of fresh water, is itself as fresh as if it came out of the garden, must be of advantage against the scurvy. Cold water answers the purpose here mentioned as well as hot, but is not quite so speedy; so that the preparation of this sallad does not, in strictness, require any consumption of fuel.

Sea water may be used.

It is an agreeable food.

Professor P. speaks with great commendation of this vegetable, when seasoned and served up under roast meat, and also as a sallad. All his friends approved of it; and the consumption of his kitchen was incomparably greater than that of his laboratory. He remarks, that a slight boiling gives it all the tenderness

tenderness it is capable of, but does not take out its bitterness, which requires a little longer time: but he remarks, that the bitterness is not at all disagreeable; and that if its effects be aperient, as Scopoli affirms, it would agree with many constitutions.

According to the judgment of several Americans, the boiled lichen particularly resembles the fucus, which is called luche at Lima, of which so great a consumption is made along the whole coast of Peru and Chili.

As a pound of lichen affords three pounds of boiled vegetable, and these, when dried, are reduced to two-thirds of a pound, it clearly follows, that two-thirds of this food, when taken, consist of water. Mr. P. anticipates this as an objection which might probably be made against its nutritious quality. And to this he replies, that it is probable that water may be among the substances upon which the digestive faculties act, and which, by its decomposition, may serve as food. He refers, in support of his arguments, to other articles in common use, which are liable to the same objection, such as boiled potatoes; and he asks whether, since a dozen of the whites of eggs really contain only one ounce of dried albumen, we are authorized to conclude that a man, who should have dined upon this dozen in an omlet, had not made a solid and satisfactory meal.

Observation to shew that it constitutes a solid nutriment, notwithstanding the great quantity of water absorbed.

The former part of the Professor's Memoir was confined to the domestic uses of the lichen. In his second part, he treats of its chemical examination.

Chemical examination of the lichen.

Many of our domestic plants are unfit to form a component part of soups and other liquid foods; but the lichen is eminently qualified for this use, its decoction being charged with nutritious matter. This soluble substance might, at first consideration, be classed with the gums; but it differs so considerably from these that the author thinks it forms a particular species, entitled to attentive examination.

It affords a nourishing matter to soups

It was before observed that the lichen loses one-third of its weight by boiling. But more strictly speaking, he informs us, that one hundred parts of the lichen grossly powdered, afford, by infusion in cold water, three parts of the extractive bitter principle; and after treatment with boiling water, the undissolved residue amounts to sixty-four parts when dried. Consequently the quintal of dry lichen consists of

Statement of its immediate parts as separated by water.

Fleshy, or pulpy part	. . .	64
Bitter part	. . . .	3
Unknown part	. . . .	33
		100

A new species of gum soluble in boiling water but not in cold.

The thirty-three parts consist of a nutritious matter, not soluble in cold water. The bitter principle is therefore separable from the rest by cold infusion of the pounded plant; and the Icelanders follow a more saving method than the Laplanders, who throw away the decoction.

Experiment for making the thick milk of the Icelanders with lichen.

The Professor boiled the farina of washed lichen with milk, until it appeared to be sufficiently cooked; and this, when seasoned with pepper and salt, was entitled to a comparison with rice, or millet, boiled in milk. It has a greenish colour, and is a little acid: but not more incompatible with habitual use among the poor than many other country foods, which those who are not accustomed to them, would by no means consider as dainties.

Another dish was made by seasoning the preceding milk and lichen with yolk of egg and sugar. It was much more pleasant, but certainly not superior in its nutritious qualities.

Great strength of its galatinous decotion.

The decoction of this plant is of a light yellow color, and has a slightly bitter taste. Its gelatinous quality is so predominant that one pound of dry lichen affords, by boiling, about eight pounds of liquid, which congeals by cooling. And as no more than one-third of the weight of the lichen enters into the decoction, the jelly itself is formed of one part of this peculiar gum and twenty-three parts of water. Professor P. considers this as the only vegetable matter capable of rendering so large a proportion of water gelatinous.

The water separates by standing.

The jelly of lichen exhibits some peculiar properties. When left to stand for some days, the water is seen to separate at the edges, however cool the place may be where it is kept. If the plate be inclined in different directions, the jelly breaks with a facility much greater than is seen in animal jellies, or even those of fruits; and in the cracks the quantity of water increases, as if there were but little attraction between that fluid and the jelly.

It is not at all viscid.

During the boiling of the jelly this principle forms, at the surface, transparent bulbs, which are renewed continually when skimmed off; but these bulbs, when again added

added to the fluid, do not dissolve unless it be boiling; a fact which confirms the opinion that this species of mucilage is less soluble than any with which we are acquainted in the vegetable kingdom. Unlike every one of the known gums or mucilages, this jelly, when concentrated, is not viscid, either cold or hot, and it may therefore be doubted whether it would be useful in the arts, unless for calico printing; a point which deserves to be examined*. From this want of viscidity it happens that the jelly, when dried upon plates, divides into transparent angular and brittle fragments of a deep red colour. The animal or vegetable jellies differ extremely from it in this respect, because the viscidity, which connects their parts, prevents their separating while drying.

Habitude of the dried gum with water.

If the dried gum of the lichen be thrown into hot or cold water it does not dissolve, but it softens and swells up, without becoming viscid or tenacious; but it deposits very speedily all its extractive matter, and consequently its bitterness. This property affords a means of purifying it in case it should hereafter be found of service in the arts.

The green is precipitable by galls, but the precipitate is soluble in hot water.

The infusion of galls, which has no action upon the known gums, instantly precipitates the jelly of the lichen, and affords a white mass, as with animal jellies; but there is this difference, that the new combination dissolves in hot water, and separates by cooling. In this fact we have a character of the mucilage of lichen, which seems to assimulate it with that of animal matters; but the following experiments decide otherwise.

Products by destructive distillation.

The gum of lichen, heated in a retort, is destroyed without being softened. Like gum arabic it leaves only 23 or 24 hundredths of charcoal. Its products do not differ from those of gum or starch; that is to say, they consist in water and vinegar of the same odour; but the oil appeared to be much more abundant. Potash separates from this vinegar only a few atoms of ammonia.

Nitric acid produces the oxalate.

The nitric acid readily converts it into very white oxalic acid, leaving no tallow, or yellow bitter principle, in the residue.

* The jelly of lichens was examined eight or nine years ago by Lord Dundonald, and offered for this purpose to the calico printers. N.

residue. Lichen, boiled and dried, affords no more than 21 or 22 hundredth parts of coal.

The nitric acid dissolves the boiled lichen with great facility; an effect which is not commonly seen with the ligneous vegetables. The product is oxalic acid and oxalate of lime, augmented, as seemed to the professor, by a foreign earth. The residue contains the yellow bitter principle in a very small quantity.

Habitude with potash.

Potash converts boiled lichen into a gelatinous pulp, similar to that which is afforded by farina in like circumstances. These facts appear to show, that the fleshy part of this plant is an indurated gum, less oxigenated perhaps than those which are soluble. The jelly of lichen may be used as food. The author made a very good blanc-manger, by adding a small quantity of flour, with sugar, and afterwards some milk, or emulsion of almonds. The author here makes a remark concerning the necessity of condiments or seasoning, to give flavour to this jelly; and takes notice, that the same necessity exists with regard to all gelatinous foods, whether animal or vegetable. Starch, as he remarks, is the basis of bread, and glue of soup; neither of which would be acceptable to the palate, or supportable by the stomach, without some stimulating ingredients to season them. The analogy of lichen to starch, in its want of solubility in cold water, is opposed by the difference, that it has no adhesion when dissolved; but he thinks it would be interesting to treat the lichen by an appropriate fermentation, to see whether this ferment would not separate it like starch. He likewise suggests the probable advantages to be derived from an examination of the several species of the vast family of the lichens.

Blanc monge, &c.

Whether the gummy matter would afford starch by fermentation.

XI. *On*

XI.

On the breeding and feeding Game Cocks. From Sir JOHN SINCLAIR'S *Collection of papers on Athletic Exercises.**

Questions proposed.

1. DOES the superiority of game cocks depend upon parentage? Which is of most importance, the male or the female? Is it of any consequence that the cock should arrive rather gradually at maturity? Is there a great difference, in point of strength and constitution, in game cocks of the same parentage? Do you prefer great or small bones? Parentage of game cocks.

2. When do you begin to feed the young cocks? What diet and drink do you give them, and what is the process by which they are brought to the greatest possible height of strength and spirit? Feeding.

3. When the game cocks are thus trained, how long do the effects thereof last? Are they temporary or permanent? Do game cocks thus trained live shorter or longer than others of the same species? Longevity.

4. What drugs are given to fighting cocks immediately before the main begins? Is it not usual, by giving them saffron, (or some drug, which has the same effect with opium, as used among the Janisaries, or brandy among the French soldiery), to excite an unnatural and short-lived courage? What are the effects of such drugs? and how do they manage the feeding up to this point, so as to take advantage of this momentary excitement? Medicines.

The

* The moral effect of cock-fighting is, no doubt, a subject deserving to be considered; but concerning this, as the Selector of a philosophical article, I wish to be supposed to advance no opinion at present.—N.

The following interesting Letter was received from a Clergyman.

West Ham, March 28th, 1805.

DEAR SIR,

Information respecting breeding, training, and management of game cocks.

I perceive that only on one part of your well directed Queries I am able to give you satisfaction, and that is, on what you would least expect from a D. D. and the sober vicar of a country parish: the subject to which I allude is cock-fighting. At the period of my childhood, when I ran wild, from ten to fifteen, I was a great cock-fighter, and though it is many years ago, I find my memory perfectly competent to even the minute narration of every fact.

But before I proceed, I will intrude a remark or two upon your preliminary observations. In all the theoretical part I completely coincide: indeed I was pleased to find so much harmony between your sentiments and those I lately transmitted to you, without the possibility of any previous concert between us.

I do not even question your facts, but seem to differ a little with respect to some of the inferences. With respect to the South Sea islanders, and the difference between them and the English sailors, I doubt whether there was any superiority in the training of the former, which gave them the advantage. An English sailor is, perhaps the very perfection of agility in his own way*. I do not know that the human powers can go

* An officer of a frigate who had been at the Sandwich Islands has declared, that our sailors stood no chance in boxing with the natives, who fight precisely in the English manner. A quarter-master, a very stout man, and a skilful boxer, indignant at seeing his companions knocked about with so little ceremony, determined to try a round or two with one of the stoutest of the natives, although stongly dissuaded from the attempt by his officers. The blood of the native islander being warmed by the opposition of a few minutes, he broke through all the guards of his antagonist, seized him by the thigh and shoulder, threw him up, and held him with extended arms over his head, for a minute, in token of triumph, and then dashed him on the deck with such violence as to fracture his skull. The gentlemen added, that he never saw men apparently possessed of such muscular strength. Our stoutest sailors appeared mere shrimps, compared with them. Their mode of life, constantly in vigorous action in the open air, and undebilated by the use of stimulating food or drink, may be considered as a perpetual state of training. Sir J. S.

go beyond it, in some instances, that I have seen with my own eyes; yet an English sailor, though he could probably *climb* a rope better, could not *dance* upon one, as I have seen the people at Sadler's Wells. The superiority, therefore, of the South Sea indians in wrestling, boxing, and rowing, I attribute merely to *practice*. It was also in their *own way* that Cooke's sailors contended with them. In a fair boxing-match, I have not a doubt but Mendoza or Humphries would have triumphed over at least twenty of them in succession. By the way, from what I have learned of amateurs, respecting these pugilists, no persons can lead more dissolute lives, except in the article of exercise. With this exception, that those among them who drink moderately (and moderation with them is free-living among other people) are the strongest.

Information respecting the breeding, training and management of game cocks.

On a subject where I am more at home, my observations will lead to the conclusion, that the simplest mode of living is the most conducive to bodily health and strength. Though very young when I pursued cock-fighting, from nice observation, which enabled me to judge of a good cock, and from a rational mode which I fell into of treating them, I hardly ever lost a battle, even against odds; but I will pursue the subject in your own order.

1st. There is not a doubt but that the sterling courage of an English game cock depends upon parentage. It is a maxim in the cockpit, that if a cock has, what they call a *spice* of the dunghill, though ever so remote, when he is galled by the spur he will run. I remember seeing a most famous cock, about eight years old, and who had in his time won forty battles, run at the last, when severely galled. A dunghill however fights harder for a round or two than a genuine game, whose courage is of a more temperate cast, and this very famous cock was an instance, who generally killed his antagonist with a stroke or two.

A true game cock is, however, so well known by his marks, that sportsmen will rarely be mistaken. My mother has bought a clutch of chickens at the door, and I have selected from them one or two by my eye, which have proved incomparable. One of these chickens gained ten battles in one day, the last against on old cock, double his weight, and after mine, which was but a *stag* (that is one year old) had been cut down to the ground, and was *counting out*, that is, given up for dead.

Large

Information respecting breeding, training, and management of game cocks.

Large bones are always preferred in cocks, and it is an excellence to stand high on their legs, for this gives them an advantage over those of a squat make.

2d. The best manner of bringing up game cocks, while young, is in a farm yard, in as free an air, and as much agreeable to nature as possible.

About three weeks or a month before they were to fight, I put them up, as it is called, or put them in a dark close penn, about two feet square. They are debilitated by being suffered to run among the penns, and their muscles are not firm. The first week I fed them upon barley, that is accounted a *scouring* food, but it answered best at the first period of their confinement. I fed them three times a day by measure, I cannot now ascertain the quantity, giving them very little water each time; and once a day, or once in two days, took them out to spar, or fight a few strokes with one another, with their spurs muffled. The second week, and during the most of the remainder of their confinement, I fed them on pure wheat, according to the same measure, having always regard to the state and regularity of their bowels, and giving a little barley, if they appeared costive. During the last three or four days I gave them white bread, according to the same measure, though I do not think bread was any better than wheat; and some that I fed entirely on wheat, after the first week seemed to do quite as well as those which had bread.

This was the whole of the process which I employed. I could always tell, by the firmness of the breast, whether my cocks were in order. I found them by far the strongest, without diminishing their activity, when they were *plump* but firm, without fat; and I question but they would have eaten as fine, and had nearly as much firm muscular flesh as a fowl from a London poulterer's. With this mode of management my cocks were four out of five, at least, successful.

3d. The training of the cocks, in the manner I have described, produces only a temporary effect; nor does it in the least seem to shorten their lives. I have known them live and fight at ten years old; whereas the poultry in my yard at present seldom reach that period.

4. I have heard of saffron and other drugs being given to cocks; but mine, which were plainly fed, always beat them. Opium or brandy may be necessary to Janisaries or French-

men,

men, but no dram is necessary to excite the courage of a true game cock, or a British soldier.

Information respecting breeding, training, and management of game cocks.

The Rev. Mr. P——— is a native of Yorkshire, and may possibly be able to give you some information on the breeding of horses, and the training of jockies. At all events, an application to him, mentioning my name, can do no harm, and you will find him an obliging and intelligent man. He lately sent me a letter on the culture of spring wheat, which I sent to the board.

I am, dear Sir,

With great respect, &c.

P. S. I had forgotten one fact worthy of notice; when a cock had been fought so hard that he is even *apparently dead*, I have known him restored to life by covering him up, all but his head, in a warm horse dunghill, or a common hot-bed in a garden. On this you may depend, and I have no doubt that the cocks I speak of would have died but for this treatment.

A short Account of the Manners in which Game Cocks are bred up and trained for fighting. By an experienced Feeder.

It is a general principle in breeding cocks, that large bones are not desirable, but that large muscles are. The thigh should be long, with as much muscle as possible. The legs should be of a medium length, and not short like the Bantam breed. They cannot stand too high if the thighs are long. They should be round bodied and not deep (cailled) breasted. A small head is of essential importance, and it is a good sign to be hazle-eyed with black eye-brows. The black breasted red cocks in general stand the penn better than any other sort.

Parentage is certainly of great consequence, though there is often a very material difference between cocks hatched at the same time and from the same parents. The blood principally comes from the female. The likeness or outward shape from the male. The hens of the game breed are very spirited and even violent, and will not suffer a strange cock to have any connection with them.

Breeding cocks *in and in*, or stale breed as it is called (that is keeping uniformly the same stock) is a very bad system. It reduces their size, and takes away their vigour to so great a degree,

Information respecting breeding, training, and management of game cocks.

degree, that they can hardly propagate their species, and the same is remarked in horses. If game cocks are bred in and in, they will stand to be killed without flinching, but they have not spirit or activity enough to attack their foes with any effect. If intended for fighting, they should never be crossed with dunghill fowls, for any taint of that blood makes them unfit for a long contest. The best plan is, occasionally to cross with some of the game breed of a different stock.

It is of great importance to have cocks *inwardly clean*, that is free from fat, for on that depends their being in wind. Neither race horses nor game cocks that are inwardly fat can be in wind. To give them a good constitution, it is better to keep them as much as possible in the open air, on a grass-plot, and with a gravel walk to go to. The more gravelly the soil on which they are kept the better. Yards are dangerous, more especially where horses are physicked, as the cocks may pick up what may do them mischief. Cleanliness is particularly necessary. When young, the chickens are kept with the hen under a hutch, and fed with oat groats; when they become older they get unhulled barley, which is reckoned more nourishing than oats. When they are put up to fight they are kept in small penns and fed for three or four days with the very best barley. For drink they get about a gill and a half of water per day, of as soft a quality as possible, and with a little toasted bread put into it to make it still softer. During the remainder of their stay in the penns, they are fed on one third wheat and two thirds barley, which is a nourishing diet, without being too costive. They are fed twice a-day, early in the morning, and at eight at night. Before being fed the second time, the crop is examined to see that it is quite empty and the food digested. They ought not to have before they are put into the penns, above three or four hens with them, and none after.

About four or five days before fighting they are physicked. The best medicine is about half a table-spoonful of cream of tartar made up with butter into a pill. This they can easily be made to take. The object is only to give them only two or three loose stools, which lightens them, and makes their flesh afterwards firmer. The day they are physicked they get nothing but a little warm water. Next morning they are put again on their hard feed of one-third wheat and two-thirds barley, and in the evening of that day they get a *hot meal*, consisting

ing of wheat bread and milk, with a little white sugar candy. More than one meal of that sort would make them heavy or lumpy. In the summer season, after being physicked, they get air the second day, but in the winter they ought to be kept warm, without being at the same time too hot.

Information respecting breeding, training, and management of game cocks.

Brandy, or any heating drug on the day of fighting, does more harm than good. They may get, however, just before they set to, a few barley corns, with a little real sherry wine.

A cock's first battle is his best, and a cock first penned, of equal goodness, will beat a double penned one.

Game cocks live fully as long as common fowls. In some cases they have lasted above fourteen years, and as sound as the first day. They are so hardy that they can be reared in the winter time much better than the dunghill sort. The cross between a game cock and a dunghill hen is excellent eating either as chickens or fowls.

XII.

Observations on the Culture, Properties, and comparative Strength of Hemp, and other vegetable Fibres, the Growth of the East Indies. By Dr. William Roxburgh *.

(Continued from Page 47 of our XIth Volume.)

Course of experiments.

To prove the durability of the various materials formerly mentioned, I had recourse to maceration in fresh water, during the hot season. The result of these trials will be found in the following table, which, in a great measure, corresponds with the former, showing the comparative strength of the various cords mentioned therein, by weights suspended by four feet lengths of them. The first three columns on the left, have been explained in the first part of these observations; in this the largest cord only of each sort has been inserted. The three last on the same side express the average weight at which each sort of cord broke, after having been kept at the bottom of muddy, half putrid, stagnant pond water, from the 27th of February to the 22d of June, 1801.

From

* From the Memoirs of the Society of Arts, 1806.

Materials, or Names of the Plants which yielded them.	Average weight at which each sort of cord broke.					
	When fresh.			After 116 days maceration.		
	White.	Tanned.	Tarred.	White.	Tanned	Tarred.
1 Hemp from England . . .	105	—	—	—	Rotten	—
2 Ditto growth of India . .	74	139	45	A	Rotten	—
3 Coir	87	—	—	—	—	—
4 Ejoo	96	—	—	94	—	—
5 Robinia cannabina, ripe .	88	101	84	40	56	65
6 The same cut while blossoming	46	61	48	Rotten	68	45
7 Crotalaria juncea	68	69	60	Rotten	51	65
8 Corchorus olitorius . . .	—	—	—	—	—	—
9 Corchorus capsularis . .	67	—	—	50	—	—
10 Flax, growth of India . .	39	—	—	Rotten	—	—
11 Agave Americana	110	79	78	Rotten	Rotten	15½
12 Aletris nervosus	120	73	48	30	26	34
13 Theobroma Augusta Linn.	74	58	44	38	54	50
14 Theobroma guazuma, Hort. Cliff	52	47	45	30	39	—
15 Hibiscus tiliaceus	41	62	61	40	55	70
16 Hibiscus Manihot	61	—	—	26	—	—
17 Hibiscus mutabilis	45	53	—	Rotten	45	—
18 Hibiscus, from Cape of Good Hope	22	—	—	17	—	—
19 Bauhina, a scandent species	69	—	—	Rotten	—	—
20 The same, but differently prepared . . .	56	—	—	Rotten	—	—
21 Sterculia villosa	53	—	—	30	—	—

East India fibrous vegetables not less than European.

Stagnant fresh water, in a rather putrid state, during the hot months of March, April, May, and June, in Bengal, must be as severe a trial for vegetable fibres, as can be well found in any country. I am exceedingly glad to find that, in general, the fibres of our East India plants stood the test infinitely better than hemp from England, or of hemp or flax the growth of Bengal.

Tar preserves them.

Tar appears in general to be a better preservative than tan during the immesrion, though I was formerly inclined to think otherwise. The powers of No. 4, to resist decay, correspond with what the Dutch historian Rumphius says of it in his Herbarium

barium Amboynense. Nos. 7, 8, 9, and 13, 14, retained their strength surprisingly. No. 15, (the bark with which the inhabitants of the South Sea islands make lines), gained considerably in power in its tarred state.

In the former part of these observations it was remarked, that numerous plants, exclusive of those which yield hemp and flax, were productive of fibres apparently well qualified for the same useful purposes; and these several sorts are pointed out, some of which had been long and well known to the natives of Asia: others appeared to me to be unknown to them. Since the date of that paper, my researches have brought to light several additional objects of the same nature, and added considerably to the imperfect knowledge I then had of others. At the close of my first experiments (vol. xxii. page 395-6) mention is made of the strength of sun cords being greatly increased while thoroughly wet with fresh water. From 100 to 200 additional experiments have been made since that time, to illustrate this interesting fact, the result of which will be found in the two last columns of the annexed table. *Additional plants fit for cordage.*

The cords now employed were made of three single yarns; and, as formerly, by no means so equally spun, or laid, as might have been done by an expert European artist: nor must their strength be compared with those of the same material in the former table, because the cords are now made considerably stouter, and the yarns are, in general, better laid, on account of their being thicker; for I suspect that the smallness of the lines employed in the former trials rendered them somewhat less accurate than the present. *How fabricated.*

Comparative Statement of the Strength of the various Materials employed in these Experiments, both dry and wet, by Weights suspended by Four Feet Lengths of the Cords.

Names of the Plants, or Materials used, and brief Remarks thereon.	Average Weight by which each Cord broke when dry.	Average Weight by which each Cord broke when wet.	Average of Cents. by wetting the Cords.
1 Hemp, the growth of 1800, from the Company's hemp-farm, near Calcutta	158	190	20
2 Jeetee of the Rajemahl Mountaineers, the clean fibres of the bark of a new species of Asclepias	248	343	38
3 This cord is made of 15 threads of fine sail-twine from Bencoolen, the produce of a new shrubby species of nettle (Urtica)	240	278	16
4 Sun (Crotalaria juncea) cut before the plants were in blossom, and steeped immediately	112	158	41
5 The same, and cut at the same time; but the plant was dried, or rather kept for some days before it was steeped	60	78	30
6 The same, cut when in full blossom, and steeped immediately .	130	185	42
7 The same as No 6, and cut at the same time, but attempted to be cut before it was steeped	100	166	66
8 The same, cut when the seed was perfectly ripe, and steeped immediately	150	203	35
9 The same as No. 8, and cut at the same time, &c. as No. 5 and 7 .	110	163	48
10 Sun, the winter crop; the seed from the coast of Coromandel, cut when the seed was ripe, and steeped immediately	160	209	31
11 A variety of Corchorus capsularis, Teetah-paat of the Bengalese, delicately fine like flax	143	146	2
12 Reddish Corchorus capsularis; the seed imported from China; also fine and soft like flax	164	164	0

Names of the Plants, or Materials used, and brief Remarks thereon.	Average Weight by which each Cord broke when dry.	Average Weight by which each Cord broke when wet.	Average of Cents. by wetting the Cords.
13 Corchorus olitorius, Bunghi-paat of the Bengalese, and very like the two last	113	125	11
14 Robinia cannabina, Dansha of the Bengalese, cut when the seed was nearly ripe	138	145	5
15 Abroma Augusta, Hort. Kew. (No. 13 of my former paper) young shoots before the flowers opened .	100	112	12
16 The same, from ligneous plants which had ripened their seed . .	121	121	0
17 Hibiscus strictus (No. 16 of my former paper, and then mistaken for Hibiscus Manihot) cut when early in blossom	104	115	10
18 The same, but cut when the seeds were perfectly ripe	128	135	5
19 Hibiscus cannabinus, Meesta-paat of the Bengalese, cut when in flower, and steeped immediately .	115	133	15
20 The same, (Gong-kura of the Telingas) cut when the seed was ripe .	110	118	7
21 Hibiscus, from the Cape of Good Hope (No. 18 of the former paper)	116	123	6
22 Hibiscus sabdariffa, cut when in flower, and steeped immediately .	89	117	31
23 Hibiscus Abelmoschus, Kalee-Kustooree of the Hindoos, cut when in flower, and steeped immediately	107	107	0
24 Hibiscus esculentus d'Heroos of the Bengalese, cut when in seed and steeped a few days after . . .	79	95	20
25 Hibiscus bifurcatus, cut when in flower, and steeped immediately	89	92	3
26 Hibiscus pilosus, an annual, cut when in advanced flower, and steeped immediately	97	130	34
27 Theobroma guazuma, young tender shoots from the roots of small trees steeped immediately	100	140	40
28 Fibres of the foot-stalks of a very large new species of plantain, (Musa superba)	78	0	0

Remarks

Remarks on some of the Plants or Materials mentioned in the annexed Table.

Jeetee, a strong material, hitherto unknown to Europeans.

No. 2. Jeetee, of the Rajemahl-hill people, who make their bow-strings of it, which are said to last five years, though in constant use, and exposed to all sorts of weather. It is the fibre of the bark of a very extensive twining, shrubby, plant, a new species of Asclepias, discovered by my son, Mr. Wm. Roxburgh, in 1800, growing wild on the tops of the hills, in the vicinity of Rajemahl. The fibres are prepared by stripping off the bark from the tender, succulent shoots, during the rainy season, when they are full of sap, and by removing the pulpy parts with the nails, or with a piece of sharp-edged hard stick upon a board. Hitherto this beautiful, strong material, has been unknown to Europeans, and, so far as I have yet been able to learn, only employed by the people of those hilly or mountainous tracts, to make their bow-strings; consequently, it will be difficult to ascertain the quantity that may be annually procured, or the price. All I can say at present is, that four pounds weight of the clean fibres, a friend procured for me, for one rupee (half-a-crown). A drawing and description of this beautiful and useful plant, is in the possession of the Honourable the Court of Directors, under the name of Asclepias Tenacissima.

Poolay of the Malays.

No. 3. Calooee, or Battang-calooee, or Poolay of the Malays, a new, shrubby species of Urtica. The cord employed was made of fine sail-twine, sent from Bencoolen by Mr. Ewer, and was made by the Malays of that place. Its strength is very great, and the beauty and fineness of the fibre adds to its recommendation. But what quantity is procurable, or the price of it, I am at present unacquainted. The plant has, however, been introduced into the botanic-garden at Calcutta, where, in little more than one year, above one thousand plants were reared from four, which were received from Mr. Ewer, the Governor of Bencoolen. I made a drawing and description, now in the possession of the Honourable the Court of Directors, of one of these plants, which flowered in November, 1804.

The Sun plant.

No. 4 to 9. The sun, on which the experiments stated in the table were made, was sown between the 11th and 18th of June, 1801, and cut at fourteen different ages, beginning on the

the 26th of July following, before the blossoms appeared, and ending the 14th of September, when the seed was fully ripe. The fibres of these different crops were prepared in different ways; viz. by macerating for a longer or shorter time; by steeping immediately after being cut; by being half dried; or dried as well as the season would permit, as practised on the coast of Coromandel.

The average result of these various trials will be found reduced to six numbers, in the table. But it is necessary to observe, that the constant wet or very damp weather, which prevails in Bengal at this season, renders it almost impossible to dry the plant, and must injure the quality of the fibre: indeed few seasons will admit of drying the plant to any extent. Various experiments, from half a day to half a year's drying, and keeping, were made, with the view of ascertaining whether steeping immediately after the plant was pulled, or at any other period, was the best for retaining the full strength of the fibre; and I have reason to believe, that immediate steeping is to be preferred, at least in Bengal, during the rains.

No. 10. This was a few square yards of sun, reared from the seed, which was received from Ganjam, on the coast of Coromandel; and though sown with the rest of the seeds in June, did not blossom till the close of the rains in October, nor ripened its seed till January. This sort I would call Winter Sun, because what is generally cultivated in Bengal, requires only about three months from the time of sowing (middle of June) for the ripening of the seed (September). Another sort.

From the experiments made, I am led to draw the following conclusions, viz.

1st. That the fibres of this material are softer and finer when the seed is sown thick, and the plant cut as early as the flowering season, or rather before, and that they become coarser progressively, till the plant, which is annual, perishes. Conclusions or general remarks on the Sun-plant.

2d. That the fibres are at their greatest strength when the seed is ripe, which corresponds with the opinion of the natives. At this period the crop requires about one third more time to complete the maceration, than if cut at the flowering-season; it may amount to from 48 to 72 hours, according to the warmth of the water, and the state of the weather. Deep water requires more time to complete the operation than shallow water, which is generally some degrees warmer.

3d.

3d. That the sooner the plant is committed to the water, after being cut, the better: probably, because during the rains it is very difficult to dry it, and on that account the strength of the fibre will be weakened and the colour injured. Besides, in cleaning or dressing the Sun by the usual modes practised in Europe for hemp, I found, on an average, that the original quantity was reduced only one third, when the plant was steeped immediately, and nearly one half, if kept with the view of drying it before it is put into water.

4th. I found the practice of drying the plant, after maceration, and previously to the removal of the bark, as followed in Europe with hemp and flax, by no means advantageous, but prejudicial.

For an account of the plant Crotalaria Juncea, which produces the Sun, and for the method of cultivating it on the coast of Coromandel, which is different from that in Bengal, consult the Coromandel Plants, Vol. II. No. 193.

No. 11, 12, 13. As a substitute for flax, these seem to deserve attention, on account of the length, strength, and fineness of the fibre, and from the durability and strength of it, after 116 days maceration. The seed of No. 12 was brought from Canton in China, under the name of China Hemp, and grows as freely in Bengal as the sorts in general cultivation there. But while the produce was fully as great, the quality was better, which induced me to distribute the seed among the natives, and recommend the cultivation of this sort in preference to No 11 and 13.

Abroma Augusta.

No. 15 and 16 Abroma Augusta, of the Hortus Kewensis, called by the younger Linnæus Theobroma Augusta, in his Supplementum Plantarum; and Woollet Comal of the Bengalese. My remarks of the 31st of January, 1801, closed with an account of this plant. The Biggah there mentioned, or rather half Biggah (for by measurement it proved to be no more) yielded two luxuriant cuttings during the hot and rainy seasons; and a third of a more limited growth by the end of the cold season. For while the cool northerly wind prevails, during November, December and January, the plant grows but little. The quantity of clean fibre obtained from the two first cuttings, weighed 245 lb. avoirdupois; and from the third, 26lb. making together 271lb. which is a produce three times greater than the average produce of Sun from the same quantity

tity of land. As the plants, though nearly two years old, are still luxuriant, I have reason to expect that the average produce will be for many years to come as great, if not greater, than it was last year. Another great advantage in favour of the Abroma Augusta is, that the Sun requires to be drest, for convenience of stowage, prior to its being shipped, by which it loses about one third of its weight: the fibres of the Abroma Augusta are naturally clean and white, and do not, in my opinion, require dressing.

New species of hibiscus.

No. 17 and 18 is No. 16 of my former communication, and there called Hibiscus Maniho. It is, however, I am now convinced, an undescribed species from the Moluccas, which I call H. Strictus, on account of the remarkable straightness of the stem and branches. Last year, all the fibres of the few plants I then had, were only sufficient to make one line. The experiments on this beautiful material were, on that account, very limited. Nevertheless, the seed collected from these few plants produced plants sufficient to fill 40 square yards of land, and yielded 33 pounds weight of the naturally clean fibre from one cutting; and, as it is a short-lived annual, does not yield any second crop. A drawing and description of this plant is in the possession of the Honourable the Court of Directors.

Hibiscus cannabinus.

No. 19 and 20. Hibiscus Cannabinus, an annual universally known over India, and in many parts cultivated, not only for the fibres of its bark, but also for its green leaves, which are of an agreeably acid flavour, not unlike sorrel, and used by the Hindoos as a pot-herb. For a drawing and description of the plant, consult Coromandel Plants, Vol. II. No. 190.

Ejoo, a very valuable palm.

Ejoo, *No. 4, of my former memoir, in which it is observed, that this very valuable and beautiful tree is found to grow well in Bengal. Since that time I have attended particularly to its growth, and found that, on an average, each tree produces about six leaves in the year, and that each leaf yields $10\frac{1}{2}$ ounces of ejoo (the black horse-hair-like fibres † employed for

* Palma Indica venifera; 2nd Rumph, Anab. vol. i, p. 57, t. 13. Arenga saccharifera of Labillardiere. Anon. Marsden's History of Sumatra, p. 77.

† The fibres grow from the base of the footstalks (stip.) of each leaf (frond.) and embrace completely the trunk of the tree. The fibres and leaves are easily removed without injuring the tree.

for making cordage) which makes the annual produce of each tree within a fraction of four pounds*.

Besides, this palm abounds, probably, more than any other in wine, furnishing sugar and ardent spirits; and when the tree arrives at maturity, the pith of it is one of the varieties of sago-meal, used by these people in their diet. Hence we have every reason to think, that it will prove one of the most profitable trees which can be cultivated in warm countries, at least in those where it will grow freely.

I had various other plants in cultivation for further experiments, when bad health obliged me to desist and come to England. From the following I had prepared the fibres, but had not made any experiments on their strength.

Agave Tuberosa.

Agave Tuberosa of the Hortus Kewensis. The large leaves of this elegant species, which has lately been introduced into Bengal, are replete with strong white fibres, far superior in appearance to those of Agave Americana (No. 11 of my first paper.)

Musæ. The plantain, in its wild state, abounds in strong fibres more or less fine. The species which we call Coccinea, yields what is called Manilla hemp: at least it was sent to me from China as that plant.

Helicteris isora

Helicteris Isora. The inhabitants of the Malabar mountains employ this material for making twine and cordage. In Wynaad they call it Ky-walla-nara. It is strong, but rather coarse, and of a dull colour. Various species of Sida, particularly Rhombifolia, and Periplocifolia, yield uncommonly fine fibres. In Rhombifolia they are particularly delicate. Urenalobata and Sinuata also abound with them. In short, the whole of the plants of this extensive natural order, called by Linnæus, Columniferæ, and by Jussieu, Malvaceæ, are furnished with substitutes for hemp and flax.

P. S. Samples of most of the materials mentioned in this paper, I have in my possession; and it is my intention to deposit them in the East-India Company's Museum, in Leadenhall Street.

XIII.

* Some of the very best trees I have found to produce fully one pound of the fibres in each leaf.

XIII.

Some Account of a very singular and important Alum Mine near Glasgow, at present worked by Messrs. MACKINTOSH, KNOX, *and Co. Taken by Dictation from Mr.* KNOX, *by the Editor.*

AT Hurlett, near Glasgow, the works of Messrs. Mackintosh, Knox, and Co. the aluminous schistus lies 10 inches thick above a coal, at all distances from the day. It is at present worked at the depth of 30 fathoms over a coal-pit worked for three centuries, and now in work. The dip is just sufficient to keep the whole excavation quite dry; and the schistus above becomes decomposed by oxigenation, and falls down * in consequence of the working maintained during that long series, constantly in the same apartment and at the lowest point. The excavation is now at the prodigious dimensions of a mile in length, and little less in breadth. The coal stratum thus taken out, is very regularly 5 feet thick. About the year 1620, a tack or lease still extant, describes it as an extensive going work; and particular precaution is taken lest the tenant should work the whole of it out. It is upon the estate of the Earl of Glasgow; and the schistus is the same as is alluded to by Dr. Black in his lectures. The alum work is perhaps the largest single work in Great Britain, and probably in the whole world; and this article being now equal in quality to that of Italy, is exported to foreign parts instead of our manufactures being dependant on supplies from abroad as formerly.

Wonderful excavation of a mile in extent.

—of great antiquity.

The whole roof of this immense cavity being exposed to the atmospheric air, is in a state of gradual decomposition. This process is so slow, that in the long period of time before mentioned, the full roof of 10 inches is in no place gone. It flakes off by the oxigenation, and falls down; in which last situation, the oxigenation goes on upon the dry floor, and swells up the mass of a fine light spicular efflorescence to the height of three, four, and even the whole five feet of the excavation.

Curious and extensive oxigenation.

The combination of circumstances in this work, are very extraordinary. Had the schistus been disposed on the floor, instead

Singular combination of circumstances.

* In the coal excavation, one-fourth has been left as pillars to support it. They are round, and about 18 feet diameter.

stead of the roof, the oxiginated surface could not have fallen off; but would have covered the inferior portion, and put an eternal stop to the process. Had the coal work been carried on from the lower to the higher part, the wates would have been left to accumulate, and would have dissolved the efflorescence as fast as it was formed. Or, had this extensive simple apartment been abandoned as is usual in collieries, at much less periods, the same effect would have followed. And, with all these advantages, if the length of time had been less, or the extent of surface more limited, the slow process of efflorescence would have been totally inadequate to the supply of a manufactory. Or lastly, if a greater number than usual of pits had not been left unfilled, no circulation of air equivalent to the efflorescence could have followed.

Efflorescence resembling a field of corn.

In these pits is found a very singular efflorescence of sulphate of magnesia growing in fine spiculæ, about a foot in length, and covering a space of 40 or 50 yards square, like a crop of corn. It has been much injured by visitors who have trodden it down and taken parts away.

Lime stone.

Lime-stone is got at the same works. It lies over the schistus, generally about 3 feet thick, more or less. It is horizontally separated into two by a very thin seam of crystallization, in which the miners make their blast, which throws down the lower portion, and leaves the upper as the roof.

Pyrites.

Very beautiful effloresced pyrites, the residue of the coal works, are found among the decomposed schistus, and are worked with other pyrites for copperas.

Specimens of the schistus in its several states.

Schistus, No. 1.—Ten inches thick native material, very dense even with conchoidal fracture.

2.—First stage of decomposition: Dirty light brown exterially, with efflorescence, and numerous small cracks throughout, shewing the slaty texture.

3.—Third state: More split and weathered.—Many parts flaked off. White saline, dusty thin covering of efflorescence, and saline matter in the cracks evidently forcing the them asunder. The salt tastes rough, acid, and ferruginous. Slight wools, or silky appearance here and there like the flowers of Benzion.

Fourth

4.—Fourth state: Light white, or very pale greenish white mass, consisting of the silky or fine fibrous salt, intermixed with flaky fragments of the yet undecomposed schistus. In two of the specimens where the damp has operated, the efflorescent salt lies closer, is more adherent to the schistus, and is greener in some places like sulphate of iron. The salt is very soluble in water, and half the weight of the mineral in the state No. 4, is taken up by that fluid.

XIV.

Method of weaving Cloth of a surprizingly fine Quality. By Mr. William Neven.*

THE inventor acquaints the secretary that he has discovered an improvement in the art of weaving, which certainly will turn out a great national advantage.

Very fine cloth made of 256 shoots in the inch.

By this improvement cotton, linen, and silk goods, can be made much sooner and finer, than by any method yet discovered. Upon this principle he has made a small piece of plain silk cloth, from hard thrown silk in the gum, that contains the amazing quantity, of 65,536 meshes in one square inch, or 256 threads in the inch of the side, which is double the number in any cloth before made.

It is impossible for any reed-maker to make a reed half so fine as to weave such cloth upon the present principles of weaving; and even if that could be done, no weaver could make use of it: but by this method, he may weave, as fine cloth in a twelve hundred reed as by the present method in one of twenty-four hundred, and with rather less than more trouble.

He sent specimens of both silk and cotton cloth, woven upon this principle, and material advantage may be derived from this plan in making cambrics, muslins, &c.

The

* Soc Arts, 1806.

The method is very simple. More threads of the warp than usual are passed between the dents of the reed.

The method as it was explained to a Committee of the Society, consists in adding more thread of the warp within each dent or split of the reed than in the common way; for instance, that where in the common mode there are only two threads in the reed, there are upon his plan three or four.

The weft or shoot is thrown in the common way with a single thread.

When the cloth is woven and taken out of the loom, it has the appearance of being barred or striped, the cane of the reed occasioning that part of the cloth struck with it to look thinner, owing to the threads of the warp being further apart.

It comes out quite fair when wetted and pulled.

The cloth is then to be wet in water, and in that state to be repeatedly stretched across by the hands backwards and forwards corner ways; by this means the threads, which apparently formed the stripe, or close part of the cloth, separate from each other, and become diffused at equal distances. The appearance of stripes being entirely removed, the cloth becomes of unexampled fineness, and extremely regular in its texture. This operation must, in cotton fabrics, be performed before the cloth goes to the bleach-ground.

Silk goods, on being taken out of the loom, must be wet and well rubbed, as in common mode of washing, and then stretched backwards and forwards, as in the manner above directed for cotten goods.

In silk goods, the warp and weft may be both alike; in cotton goods the weft may be softer, but of the same fineness.

Mr. Neven stated, that fine linen cambrics may be made much superior to any hitherto made in France; and that though there are three threads within each dent, or split of the reed, whilst the cloath is weaving, yet the headles or yields lift up the threads alternately throughout the whole breadth of the cloth, and that there are about 250 shoots in an inch.

XV.

XV.

Extract of a Letter from Mr. H. STEINHAUER, *dated Fulneck, Jan.* 30, 1807.

IF the phenomenon below described has already attracted notice, or if you think it unworthy of it, I beg you to consign it to oblivion; but if it is worth attention, it may perhaps find its way into some corner of your valuable Journal. New kind of halo.

June 28, 1805, between 10 and 12 A.M. being off the coast of North Wales, three or four miles, and the ship going with a pleasant side-wind, about three knots per hour, I was agreeably surprised, while standing on deck, to perceive the shadow of my head in the water, environed by a luminous circle, apparently eight or nine feet in diameter, of a brilliant white colour. It appeared as if formed by the reflexion of the rays of the sun upon minute white particles in the water. However, neither sand nor perceptible globules of air could be discovered upon close examination. A similar appearance, surrounding the shadow of the head, upon the dewy grass, is occasionally observed shortly after sun-rise, but tinged with prismatic colours, and of small diameter. The circular rainbow in the spray of water-falls, bears a nearer resemblance to the above-mentioned appearance; but there was here no *perceptible* spray, and an *imperceptible* one could hardly produce the brilliant appearance. I merely mention the fact, as I do not remember to have seen it noticed, as it may perhaps serve to elucidate some hypothesis, or be applied as an example of some law, observed by an unprejudiced person.

I need hardly add, that though several viewed the phenomenon, each saw the luminous halo *only around his own head.*

XVI.

Letter to the Editor, concerning the Blacking for Leather.

SIR,

YOU will not, I trust, be offended at being consulted respecting a manufacture, humble indeed its kind, but of no small

small importance in domestic economy. I mean, blacking for shoes and boots. I have seen several recipes for liquid blacking, all of which appeared to be ill-combined farragos—all of them containing several ingredients either useless or hurtful. The bases of all were ivory black, oil, and vitriolic acid; but in such vague and indeterminate proportions, such as two-pennyworth, that there could be no certainty of a perfect saturation, nor any security of the leather from injury. Certain it is, my shoes and boots do not last so long as they were wont some twenty years ago; but whether this is owing to the corrosive quality of the new-fashioned pigment now in use, or to the more expeditious, although probably less perfect process of tanning, I am at a loss to determine. If you, Sir, or some of your ingenious correspondents will favour the writer and the public with a good recipe for liquid blacking, you will not only render a service to both, but likewise prove the means of abolishing the frequent but nefarious practice of extorting sums of money (one to five guineas) from credulous footmen and their sapient masters, for worthless or pernicious nostrums.

I remain, Sir,
Your constant reader,
and obedient humble Servant,

Bristol, 26 *Jan.* 1807. C.

Reply.

The disposition to exhibit marks of the most fastidious neatness in our clothing, is one of the characters of refined society. To give a glossy black surface to leather, when cleaned, is considered as one of the requisites for this intention; but I do not know that any of the pigments hitherto used are entitled to much commendation for the effects which the venders ascribe to them. To render leather flexible, soft, and impenetrable to water, and at the same time shining, does not seem to be practicable. Oil-grease and bees-wax, with lamp-black, or ivory-black, are the principal ingredients in the compositions for the former purpose; and the latter has usually been effected by sugar dissolved in beer or water, with the addition of the black. There does not appear to be any reason why the proportions should be considered as of any great consequence. For boots, or shoes, intended to keep out water,

a com-

a compound of wax and tallow, with lamp-black, will probably be found amongst the most useful, if laid on before the fire, in order that the pores of the leather may be closed; but the aqueous compound will be preferrable, where the mere appearrnce is regarded.

SCIENTIFIC NEWS.

Astronomy.

THE friends of astronomy will learn with pleasure, that the observatory of Seeberg, founded by the late duke of Saxe-Gotha, will not much longer remain in the deserted state it has languished in since the death of that prince. M. de Zach, who directed it, and had retired under the protection of the Dowager Duchess, will attend that princess into Italy. Consequently, he will resign his place of astronomer, which, it is hoped, will be given to the celebrated Dr. Olbers. The observations of Seeberg, after a short interval, will be renewed with new zeal, and will continue to add to the treasures of astronomy. Seeberg Observatory.

Beavers in Westphalia.

A German Journal informs us, that beavers exist in Westphalia, on the banks of the Lippe, where they maintain their situation, notwithstanding the efforts of the inhabitants to destroy them. It is easy, say the narrators, to prove their existence, by the great number of felled trees on the banks of the river. Ought we to conclude from this remark, that the beavers of this district live in society? The fact is of sufficient consequence to have deserved a more ample detail. Beavers in Germany.

Letter from the Rev. PETER ROBERTS, *A. M.*

Wrexham, Denbighshire, Dec. 27, 1806.

SIR,

In looking back over the numbers of your interesting Magazine, I find, at the end of that for January last, a notice given of "An invention laid before the Celtic Academy, of a mode "of corresponding with men, whose language is unknown, "with expedition, without any previous study, any expence, "the least trouble, or the smallest labour of the mind."

The

The little tract which accompanies this note, and which, as will appear by the date, was published in A. D. 1802. presents a mode of such correspondence, with the facilities above mentioned, in as great a degree as the subject is capable of; and as I presume you will think it just that whatever be attributed to such an invention, or the priority of invention, should be attributed where it is of right due, I take the liberty of requesting you will have the goodness to notice this in your Journal, in such manner as you shall judge to be the most proper.

Whether the mode presented to the Celtic Academy bears any resemblance to mine or not, I have not been able to learn, and can therefore make no comparison as to their relative advantages; but I hope you will allow me, Sir, to refer mine to your consideration, being persuaded that one so scientific will perceive the utility of my mode, and the ease with which it is practicable, as several others have done.

Should it appear so to your judgment, your noticing it as such will be a particular favour to,

Sir,

Your very obedient and humble Servant,

PETER ROBERTS.

The pamphlet with which the author has favoured me, is entitled " Art of Universal Correspondence," and is comprised in sixteen pages. The author's instructions are, that the correspondents be provided with a double dictionary of the two languages, and he uses marks or characters to denote the inflexions, the pronouns, and some of the most necessary of the conjugations. These characters being few in number, and simple as well as universal, are easily remembered and applied, and all the irregularities of that part of languages, which they represent are done away. The marks denote, 1st, the articles *a* and *the*, seldom used; 2d, gender, number, and case of substantives; 3d, comparison of adjectives; 4th, pronouns; 5th, tenses of verbs, and 6th, conjunctions. The ready application of these is shown by appropriate examples; in which, besides the general advantages of the system, the reduction is between one fourth and one third of the common extent of writing.

Printed by P. Da Ponte, 15, Poland-Street, Oxford-Street.

A

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AND

THE ARTS.

APRIL, 1807.

ARTICLE I.

On the Inflammable Gas formed during the Distillation of Peat. By THOMAS THOMSON, *M. D. F. R. S. E. Communicated by the Author.*

IT is well known, that when vegetable substances are exposed to heat, in close vessels, they are decomposed, and yield, among other products, a considerable portion of inflammable air, which varies in the colour of its flame, in its specific gravity, and in its other properties, according to the substance from which it has been procured, or the degree of heat at which it has been evolved.

Inflammable Gas by heat from vegetables.

The examination of these inflammable gases forms, at present, one of the most difficult branches of pneumatic chemistry. Neither their number nor constituents have been hitherto ascertained with precision; and some of the most sagacious and best-informed chemical philosophers have embraced opposite opinions respecting both. According to some, they may be all reduced to three gases, with which we are already sufficiently acquainted; while others consider them as liable to an infinity of variations, or limited only by the pro-

Their nature yet but little known.

 cesses

cesses of the operator, and the number of substances from which they are obtained. That we are already acquainted with all the inflammable gases from vegetables, which it is possible to form, is an opinion which but ill accords with the present imperfect state of chemical knowledge. But it is to be hoped, for the future progress of the science, that the opposite doctrine is equally unfounded; for, were the number of such gases indefinite, the examination of them would be not only a disgusting and hopeless task, but altogether useless and nugatory.

Three at present distinguished

Three distinct inflammable gases from vegetables, are at present known, and characterized with considerable precision. These are,

I. Carbonic oxide.

I. *Carbonic Oxide*, first accurately examined and analyzed by Mr. Cruikshanks, and lately shewn by Mr. William Henry, to be not unfrequently produced during the distillation of vegetable substances. It is characterized by a specific gravity, nearly equal to that of common air, by the blue flame with which it burns, the small quantity of oxygen which it consumes, and the great proportion of carbonic acid which it forms.

II. Carbureted hydrogen.

II. *Carbureted Hydrogen*, a gas which rises spontaneously from marshes in hot weather. Its specific gravity is exactly 6-10ths of that of common air: it burns with a white flame, consumes twice its bulk of oxygen gas, and forms exactly its own bulk of carbonic acid.

III. Olefiant gas.

III. *Olefiant Gas*, or *Supercarbureted Hydrogen*, a gas which is procured by distilling a mixture of four parts sulphuric acid and one part alcohol, and which Mr. William Henry has shown to be evolved in great quantities during the combustion of pit-coal. It is characterized by the property which it has of losing its gaseous form, and assuming that of an oil, when mixed with three times its bulk of oxymuriatic acid gas. Its specific gravity is 9-10ths of that of common air. It burns with a yellowish white flame, like oils, and emits more light than any other gas. It consumes three times its bulk of oxygen gas, and forms twice its bulk of carbonic acid.

Opinion of chemists as to their nature.

The first of these gases is considered at present, by the greater number of chemists, as a compound of carbon and oxygen, the two last as compounds of carbon and hydrogen, differing from each other in the proportion of carbon; the first containing

containing the smaller, the second, the greater proportion of that constituent.

If any confidence is to be put in the experiments immediately to be detailed, the gas which forms the subject of this paper constitutes a fourth species of inflammable air from vegetables, to which the name of *oxycarbureted* hydrogen may be given, a name already applied by Berthollet to carbonic oxide, but to which the gas from peat is much better entitled. If I shall be so unfortunate as to fall into error, I hope the difficulty of the subject will, in some measure, constitute an apology.

New gas: oxycarbureted hydrogen.

Hitherto the gas obtained during the distillation of peat, has been examined only, as far as I know, by Mr. William Henry, of Manchester. But as the properties of his gas differ essentially from those of mine, it is very clear that the gases must have been of a different nature. Indeed, I have ascertained, by experiment, that different kinds of peat yield different kinds of gas, though I have not been so fortunate as to form any gas possessed of the properties which he describes; doubtless, because the peat which he employed was very different from any which I could procure.

Gas from peat, examined by Henry.

The peat used in all my experiments was the kind commonly sold in Edinburgh: its quality was very indifferent; for it was soft, brown-coloured, and very spongy, and loose in its texture. Its specific gravity was only 0.600. When kept at the tenperature of 300°, it lost 1-4th of its weight. Between 400° and 500°, it smoked, and was charred, emitting the usual vapour of burning peat. When heated to redness, in close vessels, it left a very brittle charcoal, amounting to 1-4th of its weight. When burnt in the open air, it left a quantity of yellowish grey ashes, containing iron, amounting to 1-100th part of the peat. Good peat is much denser, not so easily decomposed, and approaches more closely to coal.

The gas obtained by the Author was different. Characteristic of the peat.

With this peat cut into small fragments, I sometimes filled an unglazed earthen retort, sometimes a cast-iron bottle, and then exposed these vessels respectively to a degree of heat which was purposely varied in the different processes. Sometimes the peat was kept for a considerable time at a temperature not exceeding 500°; and when all gas had ceased to come over, it was raised to a red heat. Sometimes it was placed at once in a strong red heat, and sometimes it was never allowed

Experiments and observations on the inflammable gas from peat.

Experiments and observations on the inflammable gas from peat.

to become red during the whole process. These variations were intended to ascertain how far the nature of the gas depended upon the temperature. But the results were not quite satisfactory. Sometimes the gas was the same, though the heat differed; and sometimes the gas varied, though all the circumstances of the process were as exactly as possible the same. The differences I am disposed to ascribe to variations in the properties of the peat employed. The gas began to come over very speedily. At first it was mixed with much carbonic acid; but the proportion of this gas diminished as the process advanced, though in one instance only it disappeared completely. The quantity of gas obtained from a given bulk of peat was much smaller than what is yielded by the same bulk of wood or pit-coal, owing probably to the great difference of weight between them.

I never succeeded in procuring the gas perfectly pure, as, besides the carbonic acid already mentioned, it always contained a portion of common air, varying from 1-8th to 1-4th of the mixture, according to the process. It was always greatest when the cast-iron bottles were used, and least with the stone-ware retorts; owing partly to the smaller size of the former, which did not allow me to throw away so great a proportion of the gas which first came over. The presence of common air cannot well be accounted for on any other supposition, than that the vessels were not altogether air-tight; for the tubes which conveyed the gas to the water-trough were very well filled. The stone-ware retorts are known already not to be impervious to air.

To remove the carbonic acid, I at first washed the gas in a large quantity of water; but finding afterwards that a portion of carbonic acid still remained, notwithstanding this process, I removed it, by washing the gas in lime-water.

To ascertain the proportion of common air contained in the gas, I employed nitrous gas, according to the method of Mr. Dalton, after having convinced myself of the accuracy of that method by repeated experiments. Into a long narrow tube, graduated to 100ths of a cubic inch, a portion of the gas to be examined, is introduced, and its bulk being noted exactly, a determinate quantity of nitrous gas, previously measured in a similar tube, is let up to it. If any common air be present, the bulk of the two gases gradually diminishes. The diminution

tion of bulk, whatever it may be, is noted down, and multiplied by 0.36842: the product is equal to the measures of oxygen present in the inflammable gas. This quantity being multiplied by 5, gives the bulk of common air mixed with the gas very nearly.

Experiments and observations on the inflammable gas from peat.

By this process, I ascertained that the gas procured by the first distillation of peat, upon which the greater number of experiments were made, was a mixture of

Inflammable gas	88
Common air	12
	100

or it contained 12 per cent. of common air.

1. This gas had a peculiar empyreumatic smell, similar to that obtained from pit-coal and from vegetable substances in general by distillation. It was not deprived of this smell by agitation in pure water or lime-water. But after washing the gas in liquid oxymuriatic acid, I could no longer perceive it. This smell is usually ascribed by chemists to a small quantity of empyreumatic oil held in solution by the gas; an opinion not yet verified by any direct experiment.

2. It is not sensibly diminished by standing over water: oxymuriatic acid gas does not immediately produce any change on its bulk; a proof that it contains no sensible quantity of olefiant gas.

3. It is extremely deleterious to animals when drawn into the lungs. Some years ago, wanting to empty a large air-holder filled with gas evolved during the distillation of wood (which is probably similar to the gas from peat), I inadvertently applied my mouth to the pipe, to draw out the gas with more rapidity. The consequence was, that after about two inspirations, I dropt down on the floor insensible, and my servant, who supposed me dead, ran out in a fright for assistance, and had returned again before I recovered. On coming to myself, I recollected applying my mouth to the stop-cock, but was conscious of no uneasy sensation whatever previous to fainting. The recovery, however, was attended with very unpleasant sensations, which continued in some measure during the rest of the day.

4. Its specific gravity was 0.8358, that of common air being reckoned 1.000. To see whether the gas altered its nature

Experiments and observations on the inflammable gas from peat.

by keeping, it was left a month standing over an open trough of water. Its specific gravity was now found to be 0.8354, or about 1-2000th less than when newly deprived of its carbonic acid. Though these experiments were made with as much care as possible, I think it not unlikely that at least a part of this small difference may be owing to errors committed in weighing the air.

As the gas was not pure, but contained 12 per cent. of common air, it is obvious that it would have been lighter, if the air had been altogether absent. It is now perfectly established, that two gases, when mixed, do not sensibly change their bulk, unless they have the property of combining intimately, and of forming a new gas, which is not the case with the gas from peat and common air. We may therefore, from the preceding experiment, deduce the specific gravity of absolutely pure inflammable gas, from peat, by calculation.* This method gives us the specific gravity of the pure gas, 0.8128. Hence, 100 cubic inches of it, at the temperature of 60°, would weigh 25.18 grs. under a mean barometer.

When this gas is made to issue from a narrow aperture into the open air, and a lighted taper brought in contact with it, it catches fire, and burns with a beautiful bluish red flame. When mixed with common air in any proportion whatever that will burn, and kindled in a close vessel by an electric spark, the flame is always pale blue. If it be mixed with a small proportion of oxygen, it burns with a reddish blue flame; but with its own bulk of that gas, the flame is a fine white. After the combustion, a portion of carbonic acid may always be detected in the detonating vessel. The bulk of the mixture is always diminished after combustion.

6. To form precise notions of the changes produced upon this gas, by burning it with common air and with oxygen, a considerable number of experiments were essential; for as these experiments are necessarily made upon very small quantities of gas, we can hope for correct results only by taking the mean

* Let A be the bulk of common air in the mixture; a, its specific gravity; B, the bulk of inflammable gas; x, its specific gravity, and c the specific gravity of the mixture. We have $Ac + Bc = Aa + Bx$; and $\frac{Ac + Bc - Aa}{B} = x$. In the present case, $A=12$, $a=1\cdot$; $B=88$, and $c=0\cdot8354$; from which we deduce $x=0.8128$.

mean of a great number of trials. The following was the plan which I followed.

Experiments and observations on the inflammable gas from peat.

(1) Before beginning each set of experiments, the inflammable gas was carefully examined by the method formerly described.

(2) Common air was always taken as composed of 21 parts of oxygen and 79 of azote.

(3) The purity of the oxygen gas employed, was ascertained in this manner: 20 measures of it were let up into a graduated tube, and 36 measures of nitrous gas added to them. The diminution of bulk was noted down, and multiplied by 0.36842. The product was the portion of oxygen contained in the 20 measures of oxygen gas employed. If this product amounted to 20, I concluded that my gas was absolutely pure; if it amounted to 10, I concluded that the gas contained half its bulk of azote, and so on; whatever was wanting to make up the 20 being always considered as azote. The oxygen gas employed was partly procured from manganese and partly from hyperoxymuriate of pot-ash. It was purposely employed of very different degrees of purity, in different experiments, as I wanted to ascertain the effect produced by the presence of different proportions of azote during the combustion of the gas.

(4) Thirty measures of the inflammable gas were generally employed. They were equal to 0.3 of a cubic inch. Each measure of oxygen and air was always equal to 1-100th part of a cubic inch.

(5) The gas and the oxygen were measured separately in narrow tubes, and then let up successively into a cylindrical glass tube, furnished with the requisite apparatus for passing an electric spark through it. This glass tube was previously filled either with water or mercury, according as the combustion was wanted to be over water or mercury.

(6) Immediately after the combustion, the residual gas was let up into a long narrow tube, to ascertain its bulk.

(7) It was then washed in lime-water, and the diminution of its bulk noted and abscribed to the absorption of carbonic acid gas.

(8) In some cases, the residue, thus freed from carbonic acid, was mixed with a fresh portion of oxygen gas, returned to the detonating tube, detonated a second time, the residue

Experiments and observations on the inflammable gas from peat.

measured, and then washed in lime-water, and measured again. But in the greater number of experiments, this repetition was not necessary, and therefore omitted.

(9) The residue, deprived of its carbonic acid, being put into a long narrow tube, a determinate quantity of nitrous gas was let up to it, and the diminution of bulk, if any, was noted and multiplied by 0.36842. The product was considered as equal to the measures of oxygen contained in the residual gas.

(10) This portion of oxygen, together with the azote known to have been present from the first, was subtracted from the residual gas, and the remainder, if any, considered as inflammable gas unconsumed.

(11) In some of the experiments, this method was considered as not sufficiently precise, and another was employed. The residual gas, deprived of its oxygen by nitrous gas, was washed in a saturated solution of sulphate of iron, till the whole nitrous gas was absorbed. From the remainder, the portion of azote present in the nitrous gas employed, was deducted. The residue was compared with the bulk of azote known to have been originally present. If it exceeded that quantity, the excess was considered as inflammable gas unconsumed.

From this account it will be perceived that there is some uncertainty respecting the residual unconsumed inflammable gas. We have no test to apply which can immediately indicate its presence; for it will not burn with oxygen, unless its quantity be considerable. Notwithstanding this uncertainty, by varying the proportion of oxygen, and its purity, we obtain results sufficiently satisfactory.

7. When the gas from peat is mixed with its own bulk of common air, it will not burn at all. But with two, three, four, and five times its own bulk of air, it burns. The combustion is most complete with three atmospheres. With five, the flame is extremely feeble, though most of the gas is consumed. The following table exhibits the result of my experiments. The gas used contained 12 per cent. of common air, and the experiments were made over water.

Experiments and observations on the inflammable gas from peat.

	Measures of Inflammable Gas.	Measures of Common Air.	Residue after Combustion.	Ditto, when washed in Lime water	Nitrous Gas added to Residue.	Bulk of Mixture
1	30	30	No Combustion.			
2	21	42	54	51	12	60
3	30	90	92.5	84		
4	12	60	61	59	17	55

But before we can form any estimate of the result of these experiments, it will be necessary to separate the pure inflammable gas and oxygen from the azote, and likewise to note the composition of the residual gas, as indicated by the trials. This is done in the following table.

	Measures of Pure Gas.	Measures of Oxygen.	Measures of Azote.	Total.	Residue.	Carbonic acid formed.	Residue deprived of Carbonic acid.	Compos. of Residue Oxygen.	Azote.	Gas.
1	26.4	6.8	26.8	60	No Combustion.					
2	18.5	9.32	35.18	63	54	3	51	1.10	35.18	14.72
3	26.4	19.4	74.2	120	92.5	8.5	84	6.00	74.2	3.8
4	10.6	12.8	48.60	72	61	2	59	7.72	48.6	2.68

From this table we obtain the following.

	Inflammable Gas consumed.	Oxygen consumed.	Diminution of bulk after carbonic acid is removed.	Carbonic acid forms.	
2	3.78	8.22	12	3	
3	22.6	13.4	36	8.5	
4	7.92	5.08	13	2	
	11.43	8.9	20.33	4.5	Average.
	100	77.86	177.86	39.37	Ditto per Cent.

Experiments and observations on the inflammable gas from peat.

From this table, it appears that the different experiments by no means agree with each other. In the second, for example, the oxygen which disappeared was more than double of the gas consumed, while, in the other two it was considerably less than the bulk of inflammable gas which disappeared. These anomalies, which appear at first sight irreconcileable, are in some measure accounted for by the subsequent experiments. In the mean time they shew us that common air cannot be employed in the analysis of the inflammable gas from peat.

8. If we substitute oxygen gas for common air, the combustion is more violent, and the detonation louder. If the proportion of oxygen be small compared with that of the inflammable gas, scarcely any of it can be detected in the residuary gas, after the detonation is over, though only a comparatively small portion of the inflammable gas is consumed. Hence, if we add a new portion of oxygen to the residuary gas, the mixture will detonate a second time, as loudly as at first. This double combustion continues till the oxygen amounts to about two thirds of the inflammable gas. If it be increased beyond that proportion, the residuary gas becomes incapable of burning, with what portion soever of new oxygen it may be mixed.

9. The first set of experiments were made with oxygen obtained from the black oxide of manganese. It was very impure, containing very nearly half its weight of azote. Two different quantities of this oxygen were employed. The first was composed of

57.9	oxygen
42.1	azote
100	

The second portion was composed of

47.89	oxygen
52.11	azote
100	

To those experiments, in which the first portion of oxygen was employed, the letter *a* is prefixed; while those in which the second portion was employed are distinguished by the letter *b*. The inflammable gas was the same as that employed

ployed in the preceding trials, and contained 12 per cent. of common air. The experiments were made over water. The following table exhibits the result of the experiments.

Experiments and observations on the inflammable gas from peat.

			FIRST COMBUSTION			SECOND COMBUSTION.					
		Measures of Gas used.	Measures of Oxygen.	Residue after Combustion.	Ditto, when washed in Lime water.	Measures of Oxygen added to Residuary Gas.	Residue after the 2d Combustion.	Ditto, when washed in Lime water.	Measures of Nitrous Gas added to Residue.	Bulk of Residue.	
a	1	30	8	No Combustion							
a	2	30	12	35	32	12	32	26	20	43	
a	3	30	16	33	28	16	34	28.5	21	38	
a	4	30	20	37.5	32	20	36	27	31	46	
a	5	30	24	37	30	24	42	32	29	43	
a	6	30	28	36	26.5	28	46	38	36	46	
a	7	30	32	39	27	32	59	59	44	47	No 2d Combus.
a	8	30	36	41	31				26	43	
b	9	20	40	38	28				27	34	
b	10	20	60	57	43.5				31	46	
b	11	20	80	78	61				48	46	
b	12	20	80	77	62				56	65	
b	13	20	80	77	64				49	58	
b	14	20	100	97	83				62	64	

Experiments and observations on the inflammable gas from peat.

But before we can form any correct idea of the result of these experiments, it will be necessary to state the exact quantities of pure inflammable gas, of pure oxygen and azote present in each mixture, and likewise to note the composition of the residuary gas, as indicated by the analysis. All this is done in the succeeding table.

		FIRST COMBUSTION.				SECOND COMBUSTION						
						Addition.				Residue.		
	Measures of pure Gas.	Measures of pure Oxygen.	Measures of Azote.	Residue after Combustion.	Carbonic acid formed.	Oxygen.	Azote.	Residue after 2d Combustion.	Carbonic acid formed.	Oxygen.	Azote.	Inflammable Gas.
2	26.4	7.71	7.89	35	3	6.96	5 04	32	6	1·10	12.93	11·97
3	26·4	10,03	9.57	33	5	9.28	6·72	34	5·5	4·23	16,29	7·98
4	26.4	12.35	11·25	37.5	5 5	11.60	8·40	36	9	4·42	19·65	2·93
5	26.4	14·67	12 93	37	7	13.92	10.08	42	10	6·63	23·01	2·36
6	26·4	16·09	14·61	36	9.5	16.24	11·76	46	8	10.31	26·47	1.22
7	26·4	19·31	16,29	39	12					2·10	16·29	8.61
8	26·4	21·63	17.97	41	10					5·16	17·97	7.87
9	17.6	19·70	22 70	38	10					4·05	22.70	1·25
10	17·6	29·30	33·10	57	13,5					10·50	-0.10	00
11	17·6	38·90	43 50	78	17					23·21	-3·71	0
12	17.6	38·90	43.50	77	15					19·52	-1·02	0
13	17·6	38·90	43.50	77	13					20·26	43 5	0.24
14	17·6	48·39	54·01	97	14					30·94	-1·95	0

These

These experiments are of two kinds, each of which ought to be considered separately. In the first five the oxygen was applied in small doses, and the gas underwent two successive combustions. In the last eight, the proportion of oxygen was greater, and one combustion only took place. Experiments and observations on the inflammable gas from peat.

By inspecting the first five experiments, it will appear that the inflammable gas was never entirely burned, but the residue diminished continually as the proportion of oxygen increased, and in the last of the list of them, did not exceed 1-22d part of the whole. If we examine the residual gas after the first combustion, scarcely any oxygen will be found in it: indeed, I could detect none at all, except when the proportion of oxygen approached that which limited the combustion to a single detonation. By subtracting the residual gas and the residual oxygen, after the second combustion, from the original quantities present, and by supposing the whole oxygen to disappear in the first combustion, we obtain the following table of the relative quantities of gas and oxygen consumed, and of carbonic acid formed, in these different experiments.

	FIRST COMBUSTION.			SECOND COMBUSTION.			
	Gas consumed.	Oxygen consumed.	Carbonic acid Gas formed.	Gas consumed.	Oxygen consumed	Carbonic acid Gas formed.	
2	2.29	7.71	3	12.14	5.86	6	
3	7.97	10.03	5	10.45	5.05	5.5	
4	5.65	12.35	5.5	7.82	7.18	9	
5	9.33	14.67	7	14.71	7.29	10	
6	14.51	16.99	9.5	10.67	5.93	8	
	7.95	12.35	6	11.16	6.26	7.7	Average.
	100	155.35	75.48	100	55.19	69	Average percent.

From this table, it appears that the proportion of oxygen which disappeared by the first detonation, was much greater, compared with the inflammable gas consumed, than in the second combustion. The average of the first combustions gives

155

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155 measures of oxygen to 100 of the gas, while the average of the second gives us only 55 measures of the oxygen to 100 of the gas. In the first, the oxygen consumed was one half greater, and in the second, one half less, than the inflammable gas. The first detonation was always louder than the second, and accompanied by a white flame, while, in the second detonation, the gas always burns with a blue flame. The diminutions of bulk are always greater after the second detonation than after the first.

If we examine the individual experiments, we shall find that the proportion of oxygen consumed by the first detonation, is a maximum, when the smallest quantity of oxygen present is the smallest possible, and that it gradually diminishes as we increase the dose of oxygen. Thus, in the first experiment, of all, the oxygen consumed by the first combustion was to the gas consumed as 338 : 100; whereas, in the last experiment, it was only as 117 : 100. In the second combustion, on the contrary, the proportion of oxygen consumed rather increases with the dose. In the first experiment of all, it is not quite equal to half the gas, while, in the last, it is rather more than half the inflammable gas consumed.

If we consider all these circumstances, it will appear extremely probable that the effect of the first combustion is twofold: that one portion of the gas is burnt, while another combines with oxygen without undergoing combustion, and forms either carbonic acid, or some other inflammable gas still unknown. The portion of this new gas formed, diminishes with the doses of oxygen, because the proportion of gas completely burnt increases. It was doubtless the formation of this new gas, in variable proportions, according to the dose of air employed, that occasioned the variations in the result when the experiments were made with common air.

As the whole quantity of inflammable gas was never consumed in any one of the experiments in which the double detonation was employed, and as the residual gas most probably consists, at least in part, of the new inflammable gas formed during the experiments, it is obvious that we cannot depend upon these trials for determining correctly the proportion of oxygen which the gas from peat consumes. The average of the whole of them gives us 105.22 measures of oxygen as the proportion

proportion consumed by 100 measures of the gas. But, for the reason assigned, we must consider this quantity as rather excessive.

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As to the carbonic acid gas formed, we cannot draw any inference from the quantities obtained in these experiments, because they were made over water; for that liquid always absorbs a portion of this gas. The portion absorbed is variable, though in general it bears some relation to the violence of the detonation and the diminution of bulk produced by it—being always greatest when the diminution of bulk is greatest. But the real quantity of carbonic acid gas formed, can only be ascertained by repeating the experiments over mercury. This, in the present case, was not done, because I considered all the experiments with the double detonation as incapable of determining the objects which I wanted to ascertain.

From the eight experiments in which such proportions of oxygen were employed, as consumed the greatest part of the gas, by a single combustion, we deduce the following table.

	Measures of Gas consumed.	Measures of Oxygen consumed.	Carbonic acid Gas formed.	Diminution of bulk suppposing the Carbonic acid removed.	
7	17.79	17.21	12	35	
8	18.53	16.47	10	35	
9	16.35	15.65	10	32	
10	17.60	18.80	13.5	36.5	
11	17.60	15.69	17	39	
12	17.60	19.38	15	38	
13	17.36	18.64	13	36	
14	17.60	17.45	14	37	
	17.55	17.41	13.08		Average.
	100	99.20	74.53		Average per cent.

In

Experiments and observations on the inflammable gas from peat.

In these experiments it deserves attention, that, after the proportion of oxygen employed exceeded a little that of the inflammable gas, there remained only a small portion of residual gas after the detonation, and that the whole of the inflammable gas was consumed when the oxygen was to the gas as 5 : 3, or in still greater proportions.

It deserves particular attention, that, in four of these experiments, the diminution of bulk is somewhat greater than can be accounted for by the quantity of inflammable gas and oxygen consumed. This small difference I ascribed, at first, to errors which had been committed in making the experiments. But after repeating each of them over again three or four times, with every possible precaution, the difference still continued as at first. I am disposed, therefore, to ascribe it to a small portion of the azote which was present, having combined with oxygen, and having formed nitric acid. We know that this happens when hydrogen, diluted with azote, is burnt with an excess of oxygen. The quantity is extremely small, and cannot materially affect the results: the only exception is the eleventh experiment, which does not correspond very well with the rest. The average of all these experiments gives us nearly 100 measures of oxygen gas consumed by 100 measures of inflammable gas, a proportion which cannot deviate far from the truth. The proportion of carbonic acid formed by the combustion of 100 measures of gas, is only 74.5 measures. But as the experiments were made over water, this proportion is rather too small. On repeating some of them over mercury, I obtained 80.5 measures of carbonic acid gas from 100 measures of inflammable gas consumed. These experiments then gave us the following proportions.

Gas consumed.	Oxygen consumed.	Diminution of bulk.	Carbonic acid formed
100	100	120	80

10. As the small portions of azote which disappeared in these experiments occasioned some ambiguity, I prepared some pure gas from the hyperoxymuriate of pot-ash. It was composed of

95.5 oxygen
4.5 azote

100.0

Having

Sir H. C. Englefield's method of adjusting the Transit Instrument.

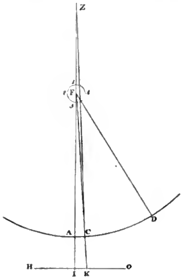

Fig. 2.

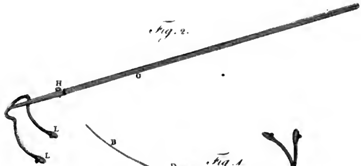

Fig. 1

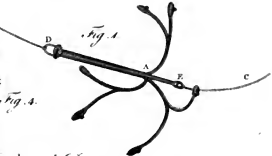

Fig. 3.

Fig. 4.

Dr. Cogan's Drags

Experiments and observations on the inflammable gas from peat.

Having exhausted my whole stock of gas from peat, I prepared an additional quantity, which, after being freed from carbonic acid, was composed of

77 inflammable gas
23 common air

100

Its specific gravity was 0.8516, which gives us, for the specific gravity of the pure inflammable part, 0.8072. This gas, of course, is a little lighter than that used in the preceding experiments. But the difference does not amount to 4 per cent. one hundred cubic inches of it, at 60°, weigh 25.02 grains. The following table exhibits a view of the experiments made with this gas and the pure oxygen.

	Measures of Gas.	Measures of Oxygen.	Residue after Combustion.	Ditto, washed in Lime-water.	Nitrous gas added to Residue.	Bulk of Residue.
1	20	20	24	20	36	27.5
2	20	30	37	30	41	18
3	20	40	46.5	38	54	22
4	20	50	55	47	74	23
5	20	60	65	58	104	32

To understand these experiments, we must, as in the former case, separate the pure gas and oxygen from the azote, and state the nature of the residual gas, as ascertained by the analysis. This is done in the following table.

	Measures of Pure Gas.	Measures of Oxygen	Measures of Azote.	Residual Gas.	Carbonic Acid.	Residual Gas.		
						Oxygen.	Azote.	Gas.
1	15.4	90	4.58	24	4	10.5	4.58	3.92
2	14.4	29.57	5.03	37	7	19.5	5.03	5.47
3	15.4	39.12	5.48	46.5	8.5	25.8	5.48	6.72
4	15.4	47.67	5.93	55	8	36.1	5.93	4.97
5	15.4	58.22	6.38	65	7	47.9	6.38	3.72

Experiments and observations on the inflammable gas from peat.

It is remarkable, that the whole of the gas was never consumed in any of these experiments, though there was present in every case a much greater proportion of oxygen than was necessary. Neither did the proportion of residue vary nearly as much as in the former case. The following table gives the proportion of gas and oxygen consumed in each experiment.

	Gas consumed	Oxygen consumed.	Carbonic acid formed.	Diminution of bulk, Carbonic Acid included.
1	11.48	9.50	4	20
2	9.93	10.07	7	20
3	8.68	13.32	8.5	22
4	10.43	11.57	8	23
5	11.68	10.32	7	22
	10.44	10.96	6.9	21.4
	100	104.98	66.1	204.98

The oxygen consumed in these experiments was greater than in the preceding. The proportion of carbonic acid is apparently less, because the experiments were made over water, and the bulk was more diminished by the combustion than in the former case. When they were repeated over mercury, I obtained an average of 8.5 measures of carbonic acid gas from the preceding proportions of inflammable gas and oxygen, which gives us 81.4 measures of carbonic acid gas for 100 of the gas from peat consumed.

The mean of these experiments and the former gives us nearly 102 measures of oxygen consumed, and 81 measures of carbonic acid formed, for every 100 measures of pure inflammable gas burnt; and these proportions I consider as approaching as near precision as we can expect to go, according to the present mode of experimenting.

11. Having

11. Having thus ascertained the properties of the gas from peat, we may easily determine whether the opinion by Mr. William Henry, be well founded, namely, that this gas is a mixture of the inflammable gases with which we are already acquainted. Experiments and observations on the inflammable gas from peat.

Of the four known inflammable gases, namely, the olefiant gas, carbureted hydrogen, carbonic oxide, and hydrogen, of which alone, from its properties, it can be a mixture, we must exclude the first, because the bulk of the gas from peat is not sensibly diminished by oxymuriatic acid. Only three hypotheses, then, can be formed; namely, 1st, that it is a mixture of carbureted hydrogen and carbonic oxide; 2d, that it is a mixture of carbonic oxide and hydrogen; or, 3d, that it is a mixture of these three gases all together. Let us examine these hypotheses.

According to the first hypothesis, our gas is a mixture of carbonic oxide and carbureted hydrogen.

The specific gravity of carbonic oxide is . .	$9560=a$
carbureted hydrogen	$6000=b$
gas from peat	$8128=c$

Let these numbers respectively be denoted by the letters a, b, and c, and let the portion of carbonic oxide in the mixture be x, and that of carbureted hydrogen, y: then, by a well-known property of fluids, we have $x:y::c-b:a-c$. Hence, since $x+y=100$, we obtain $x=59.78$ and $y=40.20$; so that if the gas from peat be a mixture of these two gases, it must be composed of

Carbonic oxide . . .	60
Carbureted hydrogen	40
	100

Now, 60 measures of carbonic oxide and 40 of carbureted hydrogen, when burnt, combine with the following proportions of oxygen, and form the following proportions of carbonic acid; and the mixture undergoes the following diminution of bulk.

 60

Experiments and observations on the inflammable gas from peat.

	Oxygen consumed.	Carbonic Acid formed.	Diminution of Bulk.
60 Carbonic Oxide	27	54	33
40 Carbureted Hydrogen	80	40	80
Total . .	107	94	113

The proportion of oxygen required by this supposition, does not differ much from that consumed by the gas from peat; but the carbonic acid is more than is formed by the gas from peat. The diminution of bulk is too small. And, upon the whole, the differences are greater than can be ascribed to errors in the experiments.

(2) According to the second hypothesis, the gas from peat is a mixture of carbonic acid and hydrogen.

The specific gravity of carbonic oxide . . $9560 = a$
hydrogen . . . . . $0843 = b$
gas from peat . . . $8128 = c$

Proportion of carbonic oxide in the mixture $= x$
hydrogen . . . . $= y$

We have, as before, $x : y :: c - b : a - c$. From this we obtain, as before, $x = 83.57$ and $y = 16.43$. So that, if this hypothesis be true, the gas from peat must be a mixture of

carbonic oxide	83.5
hydrogen gas	16.5
	100

The following table shews the oxygen consumed, the carbonic acid formed, and the diminution of bulk, when such a mixture is burnt with the requisite quantity of oxygen.

	Oxygen consumed.	Carbonic Acid formed.	Diminution of Bulk.
83.5 Carbonic Oxide	37 5	75.1	45.9
16.5 Hydrogen	8.5	0.	25.0
Total . .	46.0	75 1	70.9

Here

Here the diminution of bulk is very different from the truth, while oxygen consumed does not amount to half the real quantity. This hypothesis, then, is still less admissible than the former.

Experiments and observations on the inflammable gas from peat.

(3) The third hypothesis only remains to be examined, according to which, our gas is a mixture of carbonic oxide, carbureted hydrogen and hydrogen.

It is obvious that, according to this hypothesis, the quantity of carbonic oxide present in 100 measures of the gas from peat, can never be less than 60 measures, nor greater than 83; that the carbureted hydrogen can never amount to 40 measures, nor the hydrogen to 16. But within these limits there is an infinite number of proportions of these gases, which will produce a gas having exactly the specific gravity of the gas from peat. If, however, we make the supposition, which will be sufficiently precise for our purpose, that one of the gases shall always be present in the mixture, in such proportions as to constitute a whole number of measures, then the number of such mixture becomes limited. Thus, if we pitch upon carbonic oxide as the gas which must make a whole number of measures, then the number of mixtures will scarcely exceed 20. But it is needless to examine the products of the combustion of such mixtures, because none of them approach the properties of the gas from peat so nearly as the mixture of carbonic oxide and carbureted hydrogen. The following are a few examples.

		Oxygen consumed.	Carbonic Acid formed.	Diminution of Bulk.
Carbonic Oxide	63	28.35	46.70	34.65
Carbureted Hydrogen	34.76	69.52	34.76	69.52
Hydrogen	2.24	1.12	0·	3.36
Total . . .	100	98.99	91.46	107.53

Experiments and observations on the inflammable gas from peat.

		Oxygen consumed.	Carbonic Acid formed.	Diminution of Bulk.
Carbonic Oxide	65	29.25	58 5	35.75
Carbureted Hydrogen	31.5	63 00	31.5	63
Hydrogen	3 5	1.75	0.	5.25
Total . . .	100	94	90	104

If we were to examine all these mixtures in succession, we should find that their properties deviate more and more from those of the gas from peat, as the proportion of carbonic oxide increases, and that the mixture nearest the gas from peat is that in which there is a minimum of carbonic oxide, and of course, in which the hydrogen disappears altogether; that is to say, it is the mixture of carbonic oxide and carbureted hydrogen already examined. Thus, the presence of pure hydrogen gas, in the gas from peat, cannot be admitted; indeed, the evolution of it from a vegetable substance exposed to heat, is contrary to all analogy. But I own I was very much inclined, from the result of the preceding investigation, to consider the gas from peat as a mixture of carbonic oxide and carbureted hydrogen, and to ascribe the differences between the gas which I examined and such a mixture, to errors into which I had fallen in making the experiments. Accordingly, I repeated the experiments day after day, on purpose, if possible, to make them tally with the hypothesis. But as the result of all the trials was constantly the same, I was obliged to renounce it. Afterwards, I satisfied myself by a set of experiments, to be detailed immediately, that the hypothesis, independent of errors of experiments, is inadmissible.

12. The gas from peat, then, not being a mixture of any known gases, we must either admit it as a peculiar compound gas, different from every other previously known, or at least as containing a mixture of a peculiar and hitherto unknown gas. The first of these opinions may be admitted at present provisionally, till a more complete investigation of the inflammable gases from vegetables enable us to decide whether the second be possible.

Let

Let us endeavour, then, from the preceding experiments, to ascertain the constituents of this new gas. The reasoning from which these constituents are reduced, is founded on a hypothesis not yet strictly demonstrated, though sufficiently probable to be admitted by chemists: the hypothesis is, that when a mixture of the inflammable gas and oxygen are burnt, all that portion of both which disappears is converted into water and carbonic acid. The proportion of carbonic acid formed is known from the experiments, while the proportion of water is deduced from it in the following manner: When oxygen gas is converted into carbonic acid, its bulk is not sensibly altered; therefore, the quantity of carbonic acid formed being subtracted from the quantity of oxygen consumed, leaves a remainder of oxygen gas which entered into combustion, but did not form carbonic acid. It is presumed that the remainder went to the formation of water. It must, therefore, have combined with the hydrogen contained in the inflammable gas. Now, to obtain the weight of this hydrogen, it is only necessary to know, that when oxygen is burnt with hydrogen, it combines with very nearly twice its bulk of that inflammable gas.

Experiments and observations on the inflammable gas from peat.

Having thus obtained the quantity of carbonic acid and of water, formed by the combustion of the gas, as the carbon in the one and the hydrogen in the other were furnished by the inflammable gas, while the oxygen was furnished by the oxygen gas present, we add the weight of that carbon and hydrogen together, and compare it with the weight of the inflammable gas consumed. If the two weights are equal, we conclude that the inflammable gas was composed of the proportion of carbon and hydrogen obtained by the experiments. But if the weight of the gas be greater than that of the carbon and hydrogen, we are obliged to have recourse to a new hypothesis, and to suppose that the difference of weight is owing to a portion of oxygen and hydrogen present in the gas, which combined during the combustion, and formed water. The proportion of these two substances deduced from the hypothesis, is added to the hydrogen and carbon previously obtained: thus making up the whole weight of gas, and giving us the constituents.

From this account of the mode of analysing these gases, it is obvious, that it is liable to some degree of uncertainty. But the

Experiments and observations on the inflammable gas from peat.

the present state of chemical science does not admit of any thing more precise; for, deducing the proportion of carbon from the carbonic acid formed, I consider it as amounting to 0.28 of the weight of that gas. For the experiments of Lavoisier and Smithson Tennant appear to me much more precise than those of Morveau, which, indeed, are contradicted by the more recent experiments of Berthollet, and were not made in such a way as to be susceptible of very correct results.

As the gas employed in the preceding sets of experiments differed a little in its specific gravity, we cannot take the mean result of both. If we take the last set, we have 100 inches of the gas equal in weight to 25 grains, consuming 105 inches of oxygen, and producing 81.4 inches of carbonic acid.

81.4 inches of oxygen formed carbonic acid

23.6 went to the formation of water, and combined with about 47.2 inches of hydrogen, supposing it in the state of gas.

81.4 inches of carbonic acid contain of carbon		10.6 grs.
47.2 inches of hydrogen weigh	—	1.2
	Total	11.8
Weight of 100 inches of the gas	—	25.02
Deficiency — —	—	13.22

These 13.22 grains we suppose to have been oxygen and hydrogen present in the gas, and which combined to form water during the combustion. But water contains very nearly 1-7th of its weight of hydrogen. Hence, they are composed of

11.02 oxygen
2.20 hydrogen

13.22

These being added to the 11.8 grains formerly obtained, give us, for the constituents of the gas from peat,

11.02 oxygen
10.60 carbon
3.40 hydrogen

25.02

or,

Experiments and observations on the inflammable gas from peat.

or, per cent. 44 oxygen
42.4 carbon
13.6 hydrogen

100.0

As this gas contains three constituents, we may give it the provisional name of oxycarbureted hydrogen, till future experiments determine whether it be a mixture or a chemical compound.

13. The gas employed in the preceding experiments, though its specific gravity varied a little, was, however, pretty nearly uniform in that respect. But, in the course of my experiments on peat, I obtained portions of inflammable gas which differed very much, both in their specific gravity and in their other properties, from the gas which we have just examined. I select the following experiments as the most striking that occurred.

The peat was distilled slowly in a small iron bottle. The gas which came over was received in two different jars. The first portion that came over was found to be a mixture of

75 inflammable gas
25 common air

100

Its specific gravity was only ·7274; which gives for the specific gravity of the pure inflammable peat 0.6365. Hence, 100 cubic inches of it, at 60°, weigh only 19.73 grains.

The second portion which came over was found to be a mixture of

71.7 inflammable gas
18.3 common air

100

Its specific gravity was 0.6883, which gives us, for the specific gravity of the pure inflammable portion 0.6082. Thus the two portions of gas differed from each other in their specific gravity, and both of them were much lighter than the gas previously examined. Indeed, they approached very nearly to the specific gravity of pure carbureted hydrogen.

With the first portion of inflammable gas thus obtained, I made the following experiments. The oxygen used contained 4.5 per cent. of azote.

From

Experiments and observations on the inflammable gas from peat.

	Measures of Gas.	Measures of Oxygen.	Residue after Combustion.	Ditto, washed with Lime-water.	Nitrous Gas added.	Residue.
1	20	20	23	19	46	41
2	20	40	42	37	60	33
3	20	60	61.5	55	118	63.5

From these experiments we can easily deduce the following table.

	Measures of Gas.	Measures of Oxygen	Measures of Azote.	Residue.	Carbonic Acid.	Residual Gas.		
						Oxygen.	Azote.	Gas
1	15	20.1	4.9	23	4	8.84	4.9	5.26
2	15	39.2	5.8	42	5	23.57	5.8	7.63
3	15	58.3	6.7	61.5	6.5	40.34	6.7	7.96

It is curious that, in these experiments, the whole of the gas was never consumed—a proof that the combustion is most complete, when a considerable quantity of azote is present. It is indeed possible, though not probable, that the constant residue was incombustible. We have no means of verifying this by experiment. From the preceding table we deduce the following, which exhibits the proportion of gas and oxygen consumed, and of carbonic acid formed.

	Gas consumed.	Oxygen consumed.	Diminution of bulk, including Carbonic Acid.	Carbonic Acid formed.
1	9.74	11.26	21	4
2	7.37	15.63	23	5
3	7.04	17.96	25	6.5
Average	8.05	14.95	23	5.17
Average per cent.	100	186	286	64.30

Here

Here the proportion of oxygen consumed increased with the proportion present. The average result is very different from that obtained in the former experiments; since here 100 of gas consumed 186 of oxygen, whereas, in the former case, the gas consumed only its own bulk of oxygen. The proportion of carbonic acid gas is too small; but over mercury it amounted only to 70 for the hundred of gas. Experiments and observations on the inflammable gas from peat.

Here 70 inches of oxygen went to the formation of carbonic acid, and 116 to that of water. These last must have combined with what was equivalent to 232 inches of hydrogen.

70 inches of carbonic acid contains of carbon	9.11 grs.
232 inches of hydrogen — —	6.03
	15.14
Weight of 100 inches gas	19.73
Residue — —	4.59

This residue must be water, and composed of

0.65 hydrogen
3.94 oxygen

4.59

Hence the gas is composed of

9.11 carbon
6.68 hydrogen
3.94 oxygen

19.73

or per cent of 46 carbon
34 hydrogen
20 oxygen

The great difference between this gas and the preceding consists in the diminution of the oxygen and the increase of the hydrogen.

Now, this gas cannot be a mixture of carbonic oxide and carbureted hydrogen: its specific gravity approaches too nearly to that of the latter gas, to admit any notable quantity of the former. It cannot be carbureted hydrogen, because the proportion of carbonic acid formed during its combustion is too small to admit of that supposition.

With the second portion of inflammable gas, which had a smaller

Experiments and observations on the inflammable gas from peat.

smaller specific gravity than the first portion, the following experiments were made.

	Measures of Gas.	Measures of Oxygen.	Residue after Combustion.	Ditto, washed with Lime-water.	Nitrous Gas added.	Residue.
1	20	20	15	9	27	27
2	20	40	36	29	55	
3	20	60	56	47	113.5	59

From these experiments we obtain the following table.

	Measures of Pure Gas.	Measures of Pure Oxygen.	Measures of Azote.	Residue.	Carbonic Acid formed.	Residual Gas.		
						Oxygen.	Azote.	Gas.
1	16.34	19.83	3.83	15	6	1.2	3.83	3.97
2	16.34	38.93	4.73	36	7	20.80	4.73	3.97
3	16.34	58 03	5.63	56	9	37.40	5.63	3.97

It is remarkable that, in these experiments, the residual gas was always the same. This renders it probable that it was incombustible, and that it differed in its nature from the gas which was consumed. The following table exhibits the quantities of gas and oxygen consumed, and of carbonic acid formed, in each experiment.

	Gas consumed.	Oxygen consumed.	Diminution of bulk, including Carbonic Acid.	Carbonic Acid formed.
1	12.37	18.63	31	6
2	12.37	18.13	31	7
3	12.37	21.63	34	9
Average	12.37	19.46	32	7.3
Average per cent.	100	158.7	258.7	59.01

Here

Here the quantity of oxygen consumed is less than in the preceding experiments. On repeating the combustion over mercury, I obtained 60.63 as the proportion of carbonic acid from 100 gas consumed. Experiments and observations on the inflammable gas from peat.

Here 60 inches of oxygen went to the formation of carbonic acid and 97 to the formation of water. These last must have combined with what was equivalent to 194 inches of hydrogen gas.

60 inches of carbonic acid gas contain of carbon		7.81 grs.
194 inches of hydrogen weigh	—	5.04
	Total	12.85
	Weight of 100 inches of gas	18.85
	Residue	6

This residue must be water, and composed of

5.15	oxygen
.85	hydrogen
6.00	

Hence, the gas is composed of

7.81	carbon
5.89	hydrogen
5.15	oxygen

or, per cent. of

41.45	carbon
31.25	hydrogen
27.30	oxygen
100.00	

These experiments were not, perhaps, sufficiently numerous to ensure results that can be altogether depended on; yet, as they were made with all possible care, and some of them repeated two or three times, the errors, I think, cannot be very great.

It is obvious that this inflammable gas, especially the last portion, cannot be a mixture of carbonic oxide and carbureted hydrogen, as its specific gravity is but very little greater than the lightest of these gases. It cannot be carbureted hydrogen, because it neither consumes so much oxygen, nor forms nearly so much carbonic oxide. But as the gas from peat varies in its specific gravity and in its other properties, it is not improbable that it is a mixture of two gases which vary in their proportions. One of them may be carbonic oxide; but I think I have

Experiments and observations on the inflammable gas from peat.

have demonstrated that the other must be a gas with which we are still unacquainted, in a separate state. It must be specifically lighter than carbureted hydrogen; must contain less carbon and more hydrogen. Were we to suppose it a species of carbureted hydrogen, it would not be difficult to deduce its specific gravity and its constituents, from the preceding experiments. But if oxygen enters into its composition, as is by no means improbable, the preceding experiments do not furnish us with the requisite data. At any rate, it would be premature, at present, to enter upon any such investigation till a greater number of the inflammable gases yielded by vegetables be examined.

II.

Observations on Professor Leslie's Theory of Caloric. In a Letter from Dr. HALLIDAY, *of Halesworth,*

To Mr. NICHOLSON.

SIR,

Whether radiant caloric be propagated with great velocity.

AFTER reading Professor Leslie's very excellent treatise on heat, I confess I became a convert to his ingenious Theory of Radiant Caloric, and then instituted a few experiments, not much different from his, with the view of confirming my opinions still more. These experiments, however, which I fear were not performed with *too much* accuracy, have somewhat shaken my faith in the Professor's theory; and I am now more than ever convinced of the truth of the generally-received notion, viz. "that caloric is capable of being projected in right lines with great velocity, and that these lines obey nearly the same laws of motion as the rays of light." I shall not enter into a detail of the experiments at present, but merely state their results, and the reasons why I have been induced to alter my sentiments; and I shall only advert to those from which Mr. Leslie has inferred the theory he has formed of radiant caloric.

Mr.

Professor Leslie's Experiments.

Mr. Leslie found, that when a screen of tinfoil or even gold-leaf, which is 600 times thinner than the tinfoil, was interposed between the thermometer and the most powerful radicating surface of the heated vessel, the effect on the thermometer was completely intercepted. But that a pane of glass only intercepted four fifths of the caloric, while a sheet of paper did not intercept so much; and, in order to do away the supposition, that the effect produced on the thermometer in the experiments with the glass and paper, was owing to part of the radiant caloric passing through their substance, he observes, that this effect was only produced when the screens were placed about two inches from the heated surface, and that when about a foot from the tin vessel, the rise in the thermometer was not one thirteenth of what it was in the first position. Hence he concludes, that the calorific influence is completely arrested, and that the screen, by this, acquires heat, and, in its turn, displays the same energy as if it had formed the surface of a new canister of the corresponding temperature.

Facts and observations, to shew that radiant caloris passes through bodies.

Now, Sir, in repeating these experiments, I observed the same results, but was led to somewhat different conclusions. I conceive that the screens of glass and paper do not entirely arrest the radiant caloric, but that they allow part of it to pass, and I do so for this reason. When the screen was placed two inches from the heated surface, I observed it acquired heat not only from the rays which it had arrested, but also by *communication*; when I placed it about a foot from the canister, it had not its temperature varied at all, and therefore I conceive the effect upon the thermometer was produced *wholly* by the rays of caloric which passed through its substance.

Heat transmitted through bodies without their temperature being raised.

In the first instance, the glass screen received caloric, not only by radiation, but still more by communication from the heated surface; so that its temperature was raised, and it became capable of radicating in its turn; of course, the rays from the canister which passed through the screen, assisted by those from tha screen itself, produce a greater effect on the thermometer. But, in the second case, the screen received no caloric by *communication*, its temperature was not raised, therefore it could not, as Mr. Leslie would have it, " display any energy in causing a fluctuation, or partial swell, in the mass of air, so as to transport the heat. I was anxious to ascertain whether or not the pane of glass, when placed about a foot

distant,

distant from the heated surface, did acquire any increase of temperature; but I assure you, if so, I could not discover it by a very delicate air thermometer: so that I conclude, that the effect produced on the thermometer, in the focus of the reflection, was by the calorific rays which passed through the screen.

Experiment with a sheet of ice.

I consider Mr. Leslie's experiment with the sheet of ice as establishing nothing whatever; for here the rays are not only arrested, but absorbed; and though I am inclined to believe that some of the rays are transmitted, yet that the "frigorific rays," if I may be allowed the expression, for the sake of being understood, are more than able to counteract any effect which they could produce.

Remarks on the experiment with glass.

In the last experiment which I shall at present notice, and which Mr. L. regards as the *experimentum crucis*, I think he does more to establish the fact, that part of the radiant caloric does pass through glass, than to make good his own theory; for here we see the effect produced, when there is a certainty that none of the radiant caloric can pass, and we find that this effect is less *cæteris paribus*, by two degrees, than when there was a possibility that some part of it might pass; and if we compare that with the quantity which passed through the glass in the former experiment, we shall find that they are nearly equal; and as metallic surfaces reflect the whole of the radiant caloric, I conceive there is but little difficulty in accounting for the striking difference which he observes took place when the tin coatings of the panes of glass were on the outer side. I admit, with Mr. Leslie, that the calorific emanation is incapable of permeating solid substances which are opaque; but when *light* can pass through, I am inclined to believe that *radiant caloric* is also capable of finding its way, or, in other words, that radiant caloric is capable of passing through transparent solid substances.

General Conclusion.

Sir, I have ventured to trouble you with these rather puerile observations, with the view of drawing some of your correspondents to the subject. It is a field in which much may yet be done.

I am, Sir,

Your very obedient Servant,

ANDREW HALLIDAY, M. D.

III.

III.

Description of a Drag for raising the Bodies of Persons who have sunk under Water. By Dr. COGAN, *of Bath* *.

SIR,

FROM the Reports of the Royal Humane Society for the year 1805, I learn, that a premium is offered by the Society instituted in London, for the Encouragement of Arts, Manufactures, &c. " To the person who shall invent and produce to the Society a cheap and portable drag, or other machine, superior to those now in use, for the purpose of taking up in the best and most expeditious manner, and with the least injury to, the bodies of persons who shall have sunk under water:" and accordingly I beg leave to submit to the inspection of the Society two models. Premium for a drag.

I have long, Sir, been discontented with the construction of the drags which have hitherto been in use, both in this and in other countries. Those used in Holland are not more than three or four inches in diameter, with very long and sharp points. They cannot therefore be properly applied to a naked body; and were not the Dutch sailors and boatmen, who are most exposed to danger, very thickly clad, they might be productive of mischief. I attempted, when resident in that country, to make some improvements, by turning the points obliquely inwards, so as to catch the clothes without penetrating deep into the body; but still these were only applicable in cases where the subject fell into the water in his clothes. The drag which is now used in London is, in many respects, exceptionable; it is clumsy and dangerous. Account of the drags used in Holland, &c.

The design of establishing a Humane Society at Bath, induced me to reconsider the subject with more attention; and the result has been the construction of two drags, according to History and

* For which the Society of Arts gave the Gold Medal, 1806.

the models which are sent to you, at the desire of that Society. The consideration of economy has induced me to construct the drag, Fig 2. as it may be made at about half the price of the other, and, in some cases, be equally useful. The drag, Fig. 1. is applicable to every case, and the only objection to it is its higher price.

description of the instrument. You will perceive, by the annexed drawing, the object in view, which is to multiply the chances of laying secure hold of any part of the body, without the possibility of an injury. Had the dimensions been smaller than they are, the drag would not encompass every part of a human body; and without the partition and curvatures at the extremities, the distances would be too great, and the body of a child might fall through the intermediate spaces. By means of the sliding hooks at the ends, the instrument is adapted both to naked bodies, and those which are clothed. As bathers are naked, the sharp-pointed extremities might lacerate in a disagreeable, though not a dangerous manner: or, by entering the skin, they might impede a firmer hold. They are therefore made to recede.

But in accidents from skaiting, or in such where the subject falls into the water with his clothes on, the hooks will be of the utmost advantage, as the slightest hold will be sufficient to render the body buoyant.

The upper extremities are made both with a socket and a loop, by which they are accommodated either to a pole or a cord; or, which is still better, to both. In ponds or rivers, where accidents are most likely to happen, should they occur at a distance from the shore, no pole would be able to reach to a sufficient extent, unless the assistants were in a boat, which is not at all times at hand. In such cases a cord may be attached to the loop, and the instrument be thrown to the place where the body is supposed to lie. If the person exposed to danger should be able to swim a little, or in any way just support himself from sinking, he might possibly lay hold of the floating piece of wood, connected with the lower end of the drag by means of a rope, and thus be brought to shore. This appendage answers another purpose. In rivers particularly, the limbs of the instrument may probably catch roots of trees, &c. and can only be disengaged by pulling the drag

drag in a contrary direction, by means of the floating wood and rope.

Description of a drag to raise the bodies of drowning persons.

When I said that both pole and cord are preferable to either singly, it was for the following reason. I have found, by experiments, that a cord tied to the ring or loop, and passing through a hole made at the upper end of the pole, gives a double advantage. The drag, with a pole attached to it, of not more than 10 or 12 feet in length, may be projected several yards further than without it; and in drawing forward the drag, till the end of the pole is brought within reach of the hand, the subject may be raised above the surface of the water in the most proper direction. But a pole of 15 or 16 feet in length is unwieldy, and would even float the drag, unless it was made much heavier.

If a drag was wanted in those cases only, where it is not necessary to throw it to a distance, then Fig. 2. would answer every purpose. It is obvious that this requires a pole to be fixed in it, so that the hand may direct the projecting parts to the body, which otherwise could not always be done.

We have not as yet had an opportunity of trying these drags upon a human body; but upon an effigy made in every respect as like as possible in form to the human body, both clothed and unclothed, they have answered in the most satisfactory manner. The effigy was brought to the surface in various directions, without ence slipping from the hold.

I shall just beg leave to add, that with two drags and a boat, assistance given in time would almost ensure success. A hook catching a single thread, it is well known, will be sufficient to bring a human body to the surface of the water, or till it becomes visible: a second drag at such time might be applied to any part of the body, so as to secure a firm hold.

The workman charges the triangular drag at one guinea, the other at 12 shillings. A pole 16 feet in length was charged three shillings. The fangs were estimated at one shilling and sixpence.

I am, Sir,
Your most humble Servant,
THOMAS COGAN.

Bath, March 1, 1806.
To C. Taylor, M.D.

Reference to the Engravings of Dr. Cogan's Drag. Plate VIII. Fig. 1, 2, 3, and 4.

Fig. 1. A shows the drag complete, with two cords, B and C, attached to it; that at the top, B, is fastened to a ring at D; the bottom cord is tied to a hole in the iron at E. The six ends of the projecting branches have each a barbed claw, which can be slided forward or drawn back, as may be thought necessary. There is a hollow socket in the upper part of the drag at D, so as to admit the end of a pole to be screwed therein, whenever it may be thought useful.

Fig. 2. Is the cheaper or more simple drag, and intended only to be used with a pole G, fastened in its hollow socket by the screw H, and to be used in the manner of a rake, to bring the body to land. It has barbed claws at the extremities of its branches L L, moveable backwards and forwards, which claws slide in a groove made in the extremity of each branch.

Fig. 3. Shows one of the claws drawn upon a larger scale, screwed to one of the extremities of a branch. In this situation the screw head appears as I, on the outside of the branch, and the claw is within, and does not extend beyond the extremity of the branch.

Fig. 4. Shows the same barbed claw as its utmost extent, projecting beyond the extremity of the branch. The end of the worm of the screw, which holds it fast in that position, appears at K.

IV. Accu-

IV.

Arguments against the Volcanic Origin of Basalt, derived from its Arrangement in the County of Antrim, and from other Facts observed in that Country. By the Rev. WILLIAM RICHARDSON, *late Fellow of Trinity College, Dublin.*

Celebrare domestica facta.—HORACE.

I HAVE, in the preceding parts of this Memoir *, discussed most of the arguments that have been adduced, by different writers, to support the volcanic origin of basalt: and I have examined the facts stated by them, to try how far they apply to this question.

Facts and observations respecting the basalt in the county of Antrim; adduced to show that it is not volcanic.

I now return to my own country, which seems more copiously furnished with curious basaltic facts than any of those upon which foreign writers have dwelt so much.

The question (to us at least) is important; for it is the origin of the ground we live upon that we are inquiring into: every particle of the surface of an extensive basaltic area, having merely a thin coat of most fertile earth, slightly covering basalt strata, accumulated upon each other to a great height; and most frequently, as it were, bursting through this surface, and displaying, in perpendicular façades, the arrangement of the materials that support us.

Whether these materials, so arranged, be formed by the hand of nature, in her original construction of the world; and thus our basaltic strata (in the language of naturalists) be entitled to the appellation of *primary*: or whether this construction of our country is to be considered as produced by mighty agents, covering our quondam surface with new and *secondary* stata, poured forth from the bowels of the earth, is surely an interesting question in the natural history of our country. And as every writer who has taken up the question of the volcanic origin of basalt, and maintained the affirmative, has recurred

* From the Irish Transactions, Vol. X. The two former parts consist of An Examination of Desmarest's Memoir in the Acad. Par. 1771, and of the principal philosophers who have followed his theory.

Facts and observations respecting the basalt in the county of Antrim; adduced to show that it is not volcanic.

to the county of Antrim for proofs, I hope that I too will be allowed to extract, from the same source, such proofs as appear to me to support the negative.

In discussing this question I shall abstain from all arguments *a priori*, and limit myself to *facts* alone; of which I hope to lay before the reader several that have escaped the notice of my predecessors; feeling that I ought to make him some amends for having detained him so long in a barren discussion of opinions, and an uninteresting detection of misrepresentations.

Before I proceed to compare the circumstances in which our basaltic area resembles or differs from volcanic countries, I must answer a charge that has been brought against me. I have been told, that it is presumption in me, who never saw a volcano, to take up a question, the solution of which must depend upon an intimate knowledge both of basaltic and volcanic countries.

I first plead example; as not one of my predecessors, who have written upon this topic, has (so far as I can find) examined both volcanic countries and our basaltic one.

I have also authority for saying, that an examination of existing volcanos is not very instructive. Mr. Kirwan tells us Collini twice ascended Vesuvius, and witnessed its eruptions, but complains he got no knowledge by it. Mr. Ferber's testimony is exactly similar. And, indeed, it is plain that, in an eruption, the lighter materials first projected upwards; then falling down, and accumulating upon the weightier, that had flowed in lava, must make it very difficult to trace arrangement; and this is the surest guide in all questions relative to cosmogony.

Mr. Strange's observations on this topic are amusing: he lets out the secret without knowing it, or availing himself of it. He says, "The phænomena of recent volcanos are very "little calculated to give us instruction. A few days tour in "Auvergne, Velay, or the Venetian State, are worth a seven "years apprenticeship at the foot of Vesuvius or Ætna."

Mr. Strange was not aware, that Auvergne, Velay, and the parts of the Venetian state he alludes to, were originally basaltic countries, in which, afterwards, volcanos erupted. Here he found a rich variety of materials: for, besides the common volcanic substances, he found all the varietes of basalt,

basalt, with the matters that usually accompany them, *ochres, zeolites, chalcedonies,* and *calcareous spar;* while at Ætna and Vesuvius he met with burnt matters alone.

Facts and observations respecting basalt in the county of Antrim; adduced to show that it is not volcanic.

The points of view in which I shall compare volcanic countries, as described by the most accredited writers, with our basaltic district, so often referred to by the same authors, are:

First. The prominent features and general resemblance.

Secondly. The different arrangement of the materials in volcanic, and our basaltic countries.

Thirdly. Frequent change in the arrangement of the materials in our basaltic country.

Fourthly. Striking and radical differences between our basalt strata, and all known currents of lava.

Fifthly. Substances found imbedded in our basalt, and never in lava.

Sixthly. Different effects produced upon foreign substances (particularly calcareous), when coming in contact with basalt and with lava.

Seventhly. Divisibility of the mass into regular forms, essential to basalt, but never noticed in lava.

First. The general and leading features of volcanic countries are admitted to be *isolated mountains,* generally conic, *truncated cones, vast craters,* with *currents of lava* issuing from them, which may be traced many miles. But as all writers upon this topic candidly admit that we have nothing similar in this country, I will not press the argument, nor enquire whether their modes of accounting for the want of these features be satisfactory or not.

Secondly. If basalt be lava, and (as this theory supposes) once flowed from a volcano, we should expect to find it arranged in the same manner with the currents of lava, which are contiguous to most known volcanos. But here the difference is most striking: for, while all writers that describe volcanic countries, represent the ejected matters as confusedly arranged, and altogether a heap of disorder; with us we observe, in the disposal of our basalt, the most consummate regularity; every separate stratum preserving steadily its own place, and never breaking into that of another.

Besides, most writers admit that currents of lava are never parallel to one another: while our basalt strata, accumulated

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mulated upon each other, preserve the most steady parallelism.

When we compare our accumulations of basalt strata with accumulations of currents of lava, which have been heaped upon one another by successive eruptions, we observe a most important difference. Currents of lava have always a layer of vegetable earth between them: this is admitted by all parties. For, while those who wish to impeach the chronology of Moses, make a prodigious interval between the eruptions necessary for the formation of this layer of earth, Moses's advocates prove, from facts, that it is often formed in a much shorter time.

Interposing layers between currents of lava being thus established, we are to look if any thing similar can be observed between basalt strata: but no such thing is to be found. Our basalt strata, whether of the same or of different varieties, pass into each other *per saltum*, without interrupting the solidity of the mass, or without exhibiting a particle of extraneous matter between them *.

Thirdly. I observed, in a former Memoir, that, on our basaltic coast, nature changes her materials, and the stile of her arrangement, every two or three miles; a fact which opposes insurmountable difficulties to the position, that the basalt strata, forming this coast, are of volcanic origin. I will select two or three of these numerous little systems, and state the order in which the strata are arranged in each of them, in a vertical direction, to give the advocates for their volcanic origin an opportunity of exerting their ingenuity, by showing how they manage their volcanos, to make them produce such diversified effects.

From

* I am aware that the ochreous layers, or strata, lying between our greater basalt strata may be stated, as contradicting this position.

The nature of these ochres (common to all basaltic countries) has given rise to much controversy; which, were I to enter into now, I would be led too far from the present question. But as this fossil makes a most conspicuous figure in many parts of Antrim, I think it well entitled to a place in the *statistical survey* of that country; the basaltic part of which I have undertaken to oblige my friend, Mr. Dubourdieu.

On the present occasion I shall only say, that I accede to the conclusion which Mr St. Fond adopted, after long doubt, and much puzzling; to wit, "*That these ochres were pure basalt, altered by some chemical operation of nature, with which we are unacquainted.*"

From Dunluce to Seaport, the façade (here the base of the arrangement) is composed of strata of tabular basalt; upon which are accumulated, up to the summit of Dunmull, columna strata, mixed with others, of the variety called *irregular prismatic*.

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East from Carrickarede, the base of the façade is white lime-stone; upon which, as long as it continues perpendicular, we find ochreous and columnar strata alternating; while the hill of Knocksoghy, above, is an uniform alternation of columnar and irregular prismatic.

The strata, forming the promontory of Bengore, are more irregularly mixed: six of tabular basalt, five columnar of four different varieties, three ochreous, and two irregular prismatic, sixteen in all: of which, after the tabular that forms the base, no two of the same kind are contiguous to each other.

The volcanist will see that he must find a distinct volcano for every separate little system surrounding our area; and that he must make the same crater emit different varieties of lava, and frequently by alternation.

Fourthly. An examination of our basalt strata, taken separately, and so compared with distinct currents of lava, will, I apprehend, turn out as little favourable to their volcanic origin as the comparison of their masses appear to do.

Whoever has read Mr. Desmarest's Memoir, or even my quotations from it, must admit that, if his theory be well founded, all our basalt strata must have once been currents of liquid lava, and, of course, should resemble those known to have issued from existing volcanos. But, I apprehend, instead of similarity, the most decided differences will be found between them.

Currents of lava, we are told, are always narrower and deeper, in the vicinity of the crater, broader and shallower, as farther removed from it: but our basalt strata are of uniform thickness in their whole extent.

There is another point of view in which the difference between basalt strata and currents of lava is still more decided. Sir William Hamilton, Ferber, Spalanzani, and even Mr. Desmarest himself, informs us, that, in all currents of lava, the materials composing them are invariably arranged, in a regular gradation, according to their specific gravities: thus, at the lowest

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lowest part of the current, compact lava, then cellular lava, then scoria, next cinders, and lastly, volcanic ashes. But, in our basalt strata, nothing similar is observed: the material is uniform; both density and specific gravity the same, through the whole thickness of our deepest strata.

Fifthly. That basalt never was in fusion, appears plainly from the substances found it, and never in lava; and which, from their nature, could not have sustained the heat of a volcano.

Of these, zeolite, chalcedony, and calcareous spar, seem to abound in the basalt of all countries, but never have been noticed in unquestioned lava. The first fuses, and the third calcines, in a very moderate heat; and, though chalcedony be more refractory, yet exposed to a strong heat, it loses its beauty, and the delicacy it exhibits in its natural state. These substances are most copiously dispersed, also, through our basalts; but as this topic has already been often urged, I will pass on to substances peculiar to my own country.

A variety of basalt, found in abundance at Portrush and the Skerrie Islands, is full of pectinites, of belemnites, and, above all, of cornua ammonis: these are dispersed through the whole mass; equally abundant in the interior and on the surface. This basalt vitrifies, and the marine substances it contains calcine in the fire of a common salt-pan; of course, never could have sustained a volcanic heat.

Another fact occurs, which seems decisive against the volcanic origin of basalt. Some varieties of this fossil, contiguous to Portrush and the Giant's Causeway, upon being broken by a sledge, discover, in their interior, cavities, some filled with fresh water, others bearing evident marks of having once contained it. Of these basalts, some were of a different variety from that of the Giant's Causeway, but of similar grain and hardness; others were precisely of the same variety, *columnar, prismatic, articulated* and exactly the same in grain. At the Causeway itself, I never found any; but in some basalts very near it, on the west side, I have met with it: these had fallen from an upper stratum.

A most respectable correspondent, to whom I communicated this fact, as new in natural history, tells me, he suspects the water passed in by percolation. Determined to pay all attention

tion to any thing suggested from such high authority, I took my friend, Mr. Joy, to the spot where I used to find the water in the greatest abundance (Ballylagan). We broke several stones, and, where we found water, observed that, at first, it wet the whole fracture evenly; but, as it evaporated gradually, the wet was confined to cracks, diverging from the little cavity that had contained the water. These, therefore, we at first supposed must have been the passages through which the water had made its way: but, on attentively examining the cracks, we perceived that, as they radiated from the cavity, they diminished in breadth, and finally terminated in the solid stone; of course, that the water had not come in by them.

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Another fact seems conclusive against percolation. I never found, in our basalt, any cavities but those which contained water, or which bore evident marks of having been once filled with it. We have, therefore, this alternative:

Either the water first made its way through the compact tissue of the basalt, then collected, and dilated itself with such force, as to form rounded cavities, often larger than a pistol-bullet, which, on many occasions, it afterwards forsook:

Or, we must admit the water to have been coeval with the basalt; to which, of course, we cannot ascribe an igneous origin.

Sixthly. As we know the high state of ignition in which lava issues from a volcano, it is reasonable to expect that, when, in its course, it meets with extraneous substances, it should produce upon them such alterations as are the usual effect of intense heat, applied to these same substances. Basalt, likewise, is often found in contact with similar matters. Hence, by a minute examination of these contacts, we have an obvious mode of ascertaining, whether the basalt also had encountered them in the same state of ignition we know the lava did.

As my country, to a great extent around me, is composed of nothing but basalt and lime-stone, I have no other substance but lime-stone upon which I can make observations. This, however, I apprehend, will be found abundantly sufficient to decide the question.

About one hundred yards from the beautiful cavern, called Long

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Long Gilbert, near the eastern extremity of the calcareous façade, a mile from Portrush, we find, half way up the precipice, a vast basaltic rock, inserted in the middle of the lime-stone mass, and, at the contact, so united to the lime-stone, as to form, with it, but one solid mass.

The peninsula of Kenbaan, near Ballycastle, is the spot where basalt and lime-stone come in contact in every possible way. Pieces of lime-stone of all sizes, imbedded in the basaltic mass, and similar fragments of basalt, dispersed in like manner through the lime-stone, and, in the precipice above, strata of basalt, and lime-stone alternating. Here the opportunities of examining the contact of basalt and lime-stone are numberless; and, on every occasion, I found them united solidly; the line of demarcation correct, as if drawn by a pencil; not the least trace of calcination, such as might be expected from the calcareous matter, coming in contact with so glowing a mass, as this theory supposes our basalt to have been.*

This unexpected circumstance has somewhat embarrassed the volcanists; who, to account for it, have been driven to various exertions of their ingenuity: but not one of them seems ever to have inquired what was the result, when calcareous matters came in contact with actual lava, as it flowed. Here an obvious mode presents itself, of deciding the question, whether basalt and lava have a common origin: for, if their contacts with calcareous matter produce the same effects upon it, we have a strong presumption in favour of the affirmative. On the contrary, should the effects turn out to be totally different, we have a conclusive argument in support of the negative.

Whether this mode of bringing the question to issue did not occur to the gentlemen who support the volcanic origin of basalt, or whether they did not like to commit a favourite theory to so rude a test, I will not presume to conjecture. Direct evidence, with a view to the question, I admit I have none;

* The result of my observations, on the contacts of basalt and lime-stone, perfectly correspond with those of Mr. St. Fond, in Vivarais (Min. des Volcans, chap. 13.) Dr. Hamilton, I admit, saw things in a different point of view: but as he does not refer us to the places where he examined these contacts, I cannot bring the point to issue in my country.

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none; yet, by an attentive examination of different writers on volcanic subjects, I find pretty good light is thrown upon this topic. The evidence I will adduce, is, I confess, indirect, and the mention of the subject incidental; yet I do not, therefore, give it less weight; for, since I engaged in polemic natural history, I have discovered, that a reliance on positive assertion is not the surest mode of obtaining truth.

The first evidence I shall produce, to the effect of actual glowing lava upon calcareous substances, is that of Lord Winchelsea, whose letter to King Charles II. (quoted by Sir William Hamilton), giving an account of the great eruption of Ætna, in 1669, says: "Where the streams of lava meet with rocks and stones of the same matter (as many are), they melt and go away with the fire. Where they meet with other compositions (calcareous, no doubt), they turn them to lime or ashes."

Mr. Ferber's testimony on the subject is decisive. He gives us, in his eleventh letter, a catalogue of ejections from Vesuvius; of which No. 6 is, by his account, "white lime-stone or marble, in loose pieces, some burnt and calcined." He observes, "they are found, likewise, in the ashes and lava, and then constantly calcined and farinaceous." Again, letter 14, he says, "at Monte Albano, the lava, as well as the piperino, contain calcined fragments of lime-stone."

Tozzetti di Targioni, in his elaborate account of the mineralogical productions of his own country, confirms Ferber's testimony, as to the uniform calcination of calcareous substances.*

Since, then, glowing lava uniformly calcines the calcareous substances it comes in contact with, and basalt produces no effect whatsoever upon them, are we not to conclude, that it did

* Tozzetti is full on the subject: He says (page 448, Vol. IX) "Se materiali sieno di natura *vitrescenti*, formeranno lave *vetrine*, se *calcarei* o *apiri*, le formeranno *polverose*."

Page 250. "In essi (lave vesuviane) si vedono misti materie vetrificate, con materie calcinate, e con altri quasi non punto tocche dal fuoco."

Page 252. "Il fuoco volcanico, nelle vescere della montagna di San Fiora, abbia offeso—fuse le massolette di metalli, e calcinate o vetrificate, secondo la loro attitudine altre sostanze."

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did not encounter them in a state of fusion? which is the point in question.

Seventhly. Upon the last difference I shall mention between basalt and lava, I must dwell a little longer; both because it seems radical and essential; and also, because it lays open some new and curious facts, relative to basalt, which have hitherto escaped notice.

I allude to that property which all basalt strata, that I ever examined, have, of dividing or separating into regular forms, generally with plain sides. For that this is a principle inherent in the mass, and coeval with its original formation, is obvious, from the striking difference between the plain brown side of the figure and the irregular, granular fracture, generally blue or grey: the former an arrangement of nature, the uniform effect of a cause, with which we are unacquainted; the latter the irregular effect of a violent stroke or impulse.

If the theory we are discussing be well founded, all our basalt strata were once currents of lava, flowing from volcanos. For this we have the authority, or, rather, the assertion, of the founder, and the most accredited supporters of the opinion. In substances, therefore, by their accounts, exactly the same, and of the same origin, (for they use basalt and lava as synonymous terms,) we have a right to expect similar properties; and to look for, in lava, an internal arrangement of the mass into regular forms, conformable to what we meet with in all basalts. But nothing similar has been observed in lava, and the description of the Volvic lava is irreconcileable to this property; for we are told it breaks in all directions, *casse en tout sens:* and Mr. Desmarest himself mentions this, as a mark of distinction between it and the neighbouring basalt.

In distinguishing the varieties of lava, we have a clew to guide us. We know the process by which it was formed; and often, upon inspection, we can discover the original material, the mother stone, by whose fusion it was made. The operation itself, too, enables us to make new distinctions, from the different intensity of heat, and different gradations in cooling.

On the contrary, we get little information from inspecting the fracture of basalt. We can tell that, in some, the constituent

stituent materials are more completely blended than in others: which seems the same thing as to say, there is much difference in grain; a great interval between the coarsest and the finest. But all this is by insensible shades; no such thing as drawing lines, by which we can mark the varieties of this fossil. Even where other differences are most essential, between the varieties of basalt, inspection cannot be relied upon. For instance, the siliceous basalt, full of marine exuviæ, passes, by gradation, from a grain as fine as jasper, until it becomes indistinguishable from the Giant's Causeway stone, and even coarser.

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If we look to nature for assistance, in classing the varieties of basalt, we will be no longer at a loss: we will find, she has impressed an indelible character on each variety of this fossil; a specific figure, into which every stratum is divisible, in its whole extent, being formed, as it were, by an agglutination of similar figures;* in the same stratum, all of nearly the same degree of perfection; but, when we compare different strata, of the same variety, the perfection or neatness of the work varies, until it passes into an amorphous mass.

Nature seems to have provided, as carefully, for the preservation of the distinctive characters, of the different varieties of basalt, as she has done, to prevent confusion in the several tribes of the animal and vegetable kingdoms. We see our basalts often, by gradation, losing their own forms, but never assuming that of another variety; and, in the last stage of evanescent form, we can trace an effort to preserve their own appropriate figure. This is very observable in our columnar basalt, and in the long horizontal prisms of our whyn dykes.

I can also trace something like a generic difference, between the varieties of our basalt: for some of them have but one principle of construction, to wit, the external visible forms; into which, upon the slightest inspection, they appear to be divided: no internal construction; the fracture irregular, and generally conchoidal. The basalts of this

* I do not use the word *similar* in a strict mathematical sense; meaning no more than a strong, general likeness, so decided, that the figures of one variety cannot be mistaken for those of another.

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this class are, the columnar, the irregular prismatic, and the tabular. I have not been able to discover subordinate forms, or an internal construction, in any of these basalts.

Other varieties, on the contrary, are regularly arranged internally; the large prism breaking into smaller, sometimes to a great degree of minuteness, as in the Portrush silecous basalt. The coarse Portrush Basalt, whose prisms are mostly quadrangular, and the unarticulated pillars of Ballylagan, have likewise the same property, in an inferior degree; while the basalts of our whyn dykes have often their subordinate prisms finished with great neatness.*

But the forms into which our basaltic masses divide, are, by no means, limited to prismatic alone. The pyramid is a common figure in our whyn dykes; and the most perfect joints of the Giant's Causeway pillars, partake both of the prism and of the pyramid, and have also a mixture of curve, and plain surfaces: the latter in number equal to the denominator of the figure; while the former amounts to double that number, plus two. Thus, a pentagon joint, taken from one of our most perfect pillars, has five plain, and twelve curve surfaces; but curve surfaces are irreconcileable, either to *crystallization* or *desiccation.*†

We

* This subordinate construction is well illustrated, in a drawing of three prismatic stones, taken from a great whyn dyke, now used as a quarry, nearly two miles west from Belfast.

The constituent figure here is a triangular prism, whose angles, at the base, seem double the angle at the vertex.

My ingenious friend, Dr. M'Donnel, to whom I had mentioned the curious construction of our whyn dykes, was so struck when he saw the prismatic stones of which this dyke is formed, extracted from the quarry, that he employed a painter to make a drawing of some of them; and he was so good as to give me a copy.

† The acute angled triangular *pyramids*, which ascend from each angle of the joint, and often reach up to the middle of the incumbent one, have their insides sloped away, in an hyperbolic curve; while the grooves in the lower part of each joint adapted to receive these, with similar curvature, added to the former, make twice as many curve surfaces as the figure has angles. The concave and convex

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We have another variety of basalt, whose surfaces external, and, if I be allowed the expression, internal, are all curves: its form is round, and it is composed of concentric spheres, like the pellicles of an onion.

This variety Mr. St. Fond himself admits not to be of volcanic origin. He says (Min. des Volc. page 46), *it must have taken this configuration naturally*. Its mode of arrangement, in the places where it is found, seems still more extraordinary. It is generally imbedded in an indurated basaltic paste; in Mr. St. Fond's language, *incorporée et incastrée dans des massifs de basalte informe*. In this state, it is sometimes built in the form of a wall, of which the globular basalt is the stones, and the unformed the cement.

I have great reason to believe, that the varieties of basalt in other countries are exactly the same as in our own; and that nature has taken the same pains to keep them distinct everywhere.

The columnar basalt, of all countries, corresponds precisely with that of the Giant's Causeway, and our other groups, as appears from the sameness of their curious articulations.

Our irregular prismatic exactly answers the description of

vex bases add two more; but, by Sir Torbern Bergman's definition, crystals are bounded by *plain surfaces*.

These facts cannot be exhibited in distinct joints; for the cohesion is so strong, that the ascending pyramids invariably break off, as the joints are separated from the pillar. It is the projecting fracture that remains, which gives the joint the appearance of a *mural crown*, as was observed by the early writers on the subject.

The destruction of these ascending pyramids makes the separate joint totally different from what it was, when existing in the perfect pillar.

To illustrate all this, I give a drawing of two pillars: one, as it appears when long exposed to the air, which acts principally upon the joints; while the dilation and contraction, from heat and cold, loosens the pyramids, and separates them from the pillar.

The second pillar exactly represents the state in which they appear, where the mass is lately quarried into, and the air has not had time to operate.

I add some joints in their natural state. This nicety of construction abates, as the pillars graduate through imperfection to an amorphous mass; yet occasional traces of it are long observable.

 the

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the basalt incumbent on the columnar at Bolsena, as given by Ferber. It is obviously similar to those at La Trezza and Pont du Baume; and Mr. Mills's view of the isolated basaltic rock at Ardlun (Phil. Trans. 1790), accurately represents this variety, in many façades, near the Giant's Causeway.

Sir Joseph Banks's account of the stratum, incumbent on the columnar at Staffa, might serve for our irregular prismatic, in most places; and the moment I shewed my friend, Mr. Joy, our neat pillars at Craigahuller, he perceived the striking likeness between the stratum, incumbent on them, and that covering the grand colonnade at Staffa.

The slight accounts we have of the Scotch whyn dykes, shew, that they are formed, like our own, of horizontal prisms.

And our globular basalts, with concentric spheres, so curiously imbedded as we find them at Port Cooan, near the Giant's Causeway, and lining some whyn dykes at Belfast Lough, are precisely the same with those taken notice of by Mr. St. Fond, at Ardenne, at Cheidevant, at Montbrul; and also by Mr. Strange, in the Venetian state.

Until the advocates for the volcanic origin of basalt can discover, in lavas, something corresponding to these curious circumstances attending our basalts, can they persist in pronouncing them to be identically the same? The one (if I may be allowed to use the expression) a *factitious* substance, of known and posterior formation; the other, bearing evident marks of the hand of nature, both in its general arrangement in mighty strata, and also in its numerous varieties. For we know that nature delights in diversifying her operations, and in executing what seems to us the same work, in many different ways.

V.

Method of adjusting a Transit Instrument in a Plane of the Meridian. By Sir H. C. ENGLEFIELD, *Bart. M. P. F.R.S. &c.*

To Mr. NICHOLSON.

SIR,

LOOKING over some papers the other day, I found the inclosed, which I had drawn up some years since. I do not know that the method of placing a transit instrument, therein described, has been made public. If it has not, I think its ease and accuracy renders it not unworthy of publication. Should you be of a different opinion, you will be so good as to return it to me. Introduction.

I am, Sir,
Your obedient Servant,
H. C. ENGLEFIELD.

Tilney-Street, March 11, 1807.

Let Z (Pl. 8) be the zenith; P, the pole; HO, the horizon; ZPI, the meridian circle; ZK, a circle of altitude distant from the meridian by a small quantity IK (suppose a degree); 1 2 3 4, the diurnal circle of the pole star, whose radius is 1° 45′ nearly; and let the altitude of the pole be 51° 30′. Then, when the pole-star is on the northern meridian, its altitude 3 I, will be 49° 45′, and its zenith distance Z 3, 40° 15′; and let ACD be a part of the diurnal circle of a star whose polar distance is 46° 30′, and N. meridian altitude 5°.

Adjustment of the transit instrument, by levelling its axis, and observing the difference of time between the transit of the pole-star and of another more distant from the pole; the R. A. being given

Now, suppose a transit instrument, whose axis is accurately levelled, and of course in the meridian at Z, to point at the horizon to K, instead of I, the true meridian; then, at 3 (the altitude of the pole-star under the pole), it will point at B, and the arch 3 B will be to IK as the cosine of the altitude 3 I to radius; but 3 B, measured on the diurnal circle of the pole-star, will be the sine of its distance from the meridian to the radius P 3 or P B; and as, in small arches, the arch of a

Adjustment of the transit instrument, by levelling its axis, and observing the difference of time between the transit of the pole-star and of another more distant from the pole; the R. A. being given.

great circle, or of a small circle, or their sines, are nearly coincident, we shall have very nearly as Z 3 (the zenith distance) to P 3 (the polar distance), so is the value of 3 B, in degrees of the pole-star circle, to its value in degrees of a circle whose radius is Z 3. And as the radius Z 3 is to P 3 very nearly as 23 to 1, the error of the transit telescope, at the altitude 3 I, will be measured by a scale (if it may be so called) 23 times as great as itself.

Now, let there be another star, A, whose northern meridian altitude is as small as it conveniently can be, for example, 5°, whose polar distance is, therefore, 46° 30′, and whose right ascension is the same as that of the polar star; then, if the transit telescope be in the meridian, both these stars will pass through it at the same time; but if it be out of the meridian by the quantity IK, the star A will pass through it when it comes to C, but the polar-star not till it comes to B, when the star A is got to D, in its diurnal circle.

The value of AC being therefore found, by multiplying IK by the cosine of its altitude AI, that value, being reduced to the angular value to the radius PA, will give the time of the star A passing through the transit telescope, after the time of its passing the meridian; and the same operation being performed for the pole-star as before directed, the difference of these times will be the error in time of the transits, answering to the given deviation IK of the transit telescope. And tables having been previously constructed for such stars as shall be thought convenient, the transit telescope may, in a very short space of time, be set to the meridian, with a degree of precision unattainable by any other method.

If the star A precedes the pole-star in its passage under the pole, no tables are requisite, nor any thing necessary to be known but the exact difference of the right ascension between the two stars; for, having observed the transit of the star A (the instrument being previously brought near the meridian, suppose half a degree), then elevate the telescope to the pole-star, by moving the horizontal adjustment of the axis: keep the pole-star on the middle wire till the due interval of time between their transits is elapsed; the instrument will then be extremely near its true position; and, by repeating the observation once more, will be brought to a perfect exactness. Or, if another star, following the pole-star in its passage, be observed

served on the same evening, if the times elapsed between their transits are equal to the tabular difference or their right ascensions, which will probably be the case, the accuracy of the first placing the instrument will be immediately ascertained. Other stars near the pole may be made use of in the same manner as is here described for the pole-star, but with proportionally less advantage, as the polar distance in increased.

Adjustment of the transit instrument, by levelling its axis, and observing the difference of time between the transit of the pole-star and of another more distant from the pole; the R. A. being given.

It is also obvious, from the figure, that the transit of the pole-star above the pole, may be also used, and that with nearly, though not quite, the same advantage as the transit below the pole.

The same method may also be applied with equal ease, if the second star A pass the southern meridian instead of the northern.

The slowness of the pole-star's motion, though it renders its transit uncertain to a few seconds, cannot materially affect the accuracy of this method, as an error of ten seconds of time in the estimation of its passage, which is certainly more than can be committed, would not cause an error of a third of a second of time in the passage of stars near the equator.

Example of the Computation with the Numbers given above.

Star A.			Pole-Star.		
Sin.	IK	8 241355	Sin.	IK	8.241855
Sin.	ZA	9.998844	Sin.	Z 3	9.810316
Sin.	AC	8.240199	Sin.	3 B	8.052171
Sin.	PA —	9.860562	Sin.	P 3 —	8.484848
Sin.	APC	8.379637	Sin.	3 PB	9.567328
	APC	1° 22′ 20″		3 PB	21° 40′ 10″
In time		$5^m\ 29\frac{1}{3}^s$	In time		$1^h\ 20^m\ 40\frac{2}{3}^s$

The error of a degree, therefore, in the position of the transit telescope at the horizon, causes the star A to pass through it 5h. 29⅓s. in time later than it ought, whereas, the same error causes the transit of the pole-star to be 1h. 26m. 40⅔s. later than it ought; and the difference between these two times, viz. 1h. 21m. 11⅓s. will be the difference of the observed time of their transits, owing to the error of the position of the transit telescope, their real right ascensions being supposed the same.

VI.

VI.

Observations on the Variation, and on the Dip of the Magnetic Needle, made at the Apartments of the Royal Society, between the Years 1786 and 1805 inclusive. By Mr. George Gilpin.*

Of the Variation Compass.

Variation compass of the Royal Society.

The variation compass, used in making the following observations, is the same instrument used in former observations of the variation, and published by the Society in several volumes of their Transactions; and as a particular and accurate description of its construction was given by Henry Cavendish, Esq. F. R. S. in the LXVIth volume, it will not be necessary to say any thing here on the subject. But these observations being the first that have been communicated since the compass was put up in the Society's apartments in Somerset Place, it may not be amiss to point out its situation in the house at the time of observation, and the method pursued to attain such allowances as were proper to be made in deducing the results here given.

Its situation, and the position of the azimuth mark described.

1. The compass in the house, at the time of observation, was placed in the middle window, on the south side of the Society's meeting-room, upon a strong mahogany board $1\frac{1}{2}$ inch thick. Against the opposite building the dial-plate of a watch is fixed, making an angle with the true meridian of $31° 8',8$ to the eastward, as a mark to which the telescope of the compass was adjusted. To obtain the angle that this mark made with the true meridian, I fixed a transit-instrument on the mahogany board above mentioned, precisely in the same place where the compass had been placed, and having adjusted its telescope to the same mark, the transits of the sun and stars over a vertical circle passing through the zenith and this mark, were observed; and the angle contained between

* Philosophical Transactions, 1806.

tween the said mark and the true meridian, was found by computation to be 31° 8',3 as above.

2. For the purpose of ascertaining what error there might be, from a want of parallelism between the line joining the indices and the magnetism of the needle, and thereby to determine whether, in the usual method of observing, the indices shew the true angle which the direction of magnetism makes with the first division or zero, a great many observations were made on both ends of the needle, and with both sides of the needle uppermost (the cap of the needle being made to fit on readily on either face for this purpose), viz. north end and south end in its upright position, and north end and south end with the needle inverted, and the mean of the four giving the angle greater by 2', than that shewn by the north end in the upright position of the needle (which was the end always used in these observations), two minutes have been added to all the observations read from the instrument, as the correction for this error to angles on the east side of zero, and subtracted from angles on the west side, to obtain the true angle; which error to angles on the west side, however, only occurred when the instrument was taken out of doors, to determine the effect of the iron work of the building.

Method of ascertaining the true line of magnetic direction, by inverting the needle

3. The variation compass being placed in the house for observation, could not be supposed to be entirely out of the influence or iron; I was therefore desirous to ascertain how far that influence might extend: for the determination of which, the following method was adopted.

—and of ascertaining the influence of iron in the building;

Having caused to be sunk into the earth, to some depth, a strong post, in the wood-yard of Somerset-House, at a considerable distance from the influence of any iron, on which the compass might be placed, and from which station there was a convenient mark, at a proper distance, to which its telescope might be adjusted: I took the compass there at those times of the day when the needle was stationary, viz. morning and afternoon. Before the compass was carried out of doors, observations were made in the room; then it was taken out of doors to the above-mentioned station, for observation there; and the observations were again repeated, after the compass had been restored to its situation in the room; so that had any alteration taken place in the interval, such alteration would

have been detected; but during the whole series, no material difference occurred between the observations made in the house before, and after those taken in the yard.

—by observations made out of doors, and compared with those within.

The observations therefore made in the yard, compared with those taken in the house both before and after those taken out of it, formed the comparison for obtaining the error, or the effect of the iron-work of the room on the needle in the house, and there is reason to believe that considerable accuracy has been obtained. They are as follow:

By a mean of 20 sets, or 200 observations taken with the compass in the yard, compared with twice that number taken in the house, before and after those taken in the yard, the variation observed in the house was found to be greater than that observed in the yard by 5',4. The mean of nine sets of observations taken in the morning giving for the error 5',5; and the mean of eleven taken in the afternoon giving for the error 5',3. The variation in those tables have therefore been lessened by the above-mentioned quantity 5',4, as the error for the effect of the iron-work of the room on the needle in the house.

Coroborating fact as to accuracy.

I must not omit to mention, that of these 20 sets of observations mentioned above, nine only were made with the compass in the same situation, and eleven in that of a different one; for, after nine sets had been taken, a pile of boards was put up between the compass and the mark to which it had been adjusted, which made it necessary to remove the post on which the compass had been placed, a few feet to the westward of its former situation, to clear it from the said pile of boards; and eleven sets of observations were made from this new station, with the compass adjusted to the same mark it had been adjusted to before, and the angles that this mark made with the true meridian from each of these stations, were ascertained by placing a transit-instrument precisely where the compass had been placed, and observing the transits of the sun and stars, in the same manner as has been described in finding the angle of the mark that the compass was adjusted to in the house. And it is conceived that this accidental circumstance adds some weight to the accuracy with which these operations were performed, as the error from the two results of nine, and eleven, does not differ so much as 0',5 from each other.

Dipping

Dipping Needle.

The dipping-needle with which the observations in this communication were made, being the same instrument used in former observations of the dip, and it having also been described by Mr. Cavendish in the paper before alluded to, it will not be necessary to say any thing of its construction here. Its situation in the house was in the eastern window in the meeting-room, next the door.

The dipping-needle.

As the observations made with the dipping-needle were not affected by any other source of error than that of the iron-work of the room, in order to ascertain the quantity of error, the instrument was taken out of doors at two different times, after an interval of ten years, differently situated each time, and the observations made at both these times out of doors, compared with the observations made in the room, giving for the error 20′ more than the dip was found to be in the room, and both agreeing to one minute; that quantity has been added to all the observations made with the dipping-needle in the room for its error, as affected by the iron-work of the room.

Although a valuable paper on the diurnal variation of the horizontal magnetic needle, by the late Mr. John Canton, F.R.S. was published in the first part of the LIst volume of the Phil. Trans. for the year 1759, containing a great number of observations made at different and irregular times of the day throughout the year, yet, it appeared to me, that if the variation were to be observed at short but stated intervals of the day for one year, the results would perhaps not only prove more satisfactory in determining the times of the needle becoming stationary, but would show its progressive and regressive motions better than if observed at irregular intervals. To effect which, I imposed this laborious task upon myself for the space of sixteen months.

Canton's observations on diurnal variation. Those of the author were carried through sixteen months.

The observations contained in Table I. in sixteen pages, viz. from September 1786, to December 1787, both inclusive, are the results made at many but stated times of the day, and so disposed, that the progress, or regress, of the variation, may be readily seen by mere inspection.*

Tabulated results.

Table

* For these, on account of their length, reference must be had to the Transactions.

Table II, contains the mean monthly variation for the above-mentioned times of the day contained in Table I.

Table III, contains, besides the mean monthly true variation, and mean monthly diurnal alteration of variation, for the sixteen above-mentioned months, the mean monthly true variation, and diurnal alteration of variation for many months in the year, between the years 1786 and 1805 inclusive.

The numbers put down in Table I, are each of them a mean of five observations, and often more.

Those in Table II. depend on Table I.

As the observations from which the true variation has been given in Table III, between the years 1788 and 1805, were too numerous to be all inserted, it has been thought sufficient to give the mean monthly true variation, and mean monthly diurnal alteration of variation only; and they were determined from a mean of the observations made at those times of the day when the variation was considered least, and greatest; which variations for each month, may generally be considered as a mean of 600 observations.

Remarks on the changes of the variation, &c.

From the observations made by the late Dr. Heberden and others, about the year 1775, the variation was found to increase annually nearly 10′, since that time to the present, its rate of increase has been considered as gradually diminishing*, and for

* An exception to the progressive increase appears between the years 1790 and 1791, as the observations between these two years make it to decrease 2′ or 3′, and subsequent observations to increase again. To what this should be attributed, I am at a loss to account, unless it arose from the alteration which took place in the iron-work of the room in December 1790, four strong iron braces having been applied to the girders in the floor of the great room of the Royal Academy (which is over the Society's meeting-room), in consequence of a cracking noise made from the great pressure of a number of persons in the room during the time that Sir Joshua Reynolds was delivering a lecture: these braces were applied two on each side of, and equidistant from, the compass, the nearest about 18 feet from it. It may be proper to mention, however, that having been favoured with the variation observed both by Mr. Cavendish and Dr. Heberden, in the above-mentioned years, the alteration of the variation was by the former nearly the same as in my own, but by those of the latter, greater in both cases.

An alteration took place between the observations made with the

for the last three or four years, the alteration has been so very small, as to make it somewhat doubtful whether it may not be considered stationary, but I would not from so short a period conclude that it really is so.

From the observations of sixteen months, viz. from September, 1786, to December, 1787, both inclusive, the variation may be considered as generally stationary at or about 7 or 8 o'clock in the morning, when it is least; and about 1 or 2 o'clock in the afternoon, when it is greatest; and therefore it has been the practice in determining the true variation, put down in the tables, to take a mean of the two morning, and the two afternoon observations, made at those times, for the true variation.

Remarks on the changes of the variation, &c.

In March, 1787. The mean monthly diurnal alteration of variation was found to be 15',0; in June 19'6; in July 19',6; in September 14',8; and in December 7',6. But on a mean of 12 years observations, from the year 1793 to 1805, the diurnal alteration of variation in March was only 8',5: in June 11',2; in July 10',6; in September 8',7; and in December 3',7.

Table IV. contains the differences for 12 years, viz. from 1793 to 1805, between the observations of the variation made in the months of March, June, September, and December, or at the times of the vernal and autumnal equinoxes, and summer and winter solstices; by a mean of these 12 years, the variation appears to increase or go westward, from the winter solstice to the vernal equinox 0',80; diminishes or goes eastward from the vernal equinox, to the summer solstice 1',43; increases again from the summer solstice to the autumnal equinox 2',13; and continues nearly the same, only decreasing 0'14, from the said equinox to the winter solstice.

Table of state of the variation for 12 years, quarterly.

These differences at the time of the equinoxes and solstices have been noticed by M. Cassini, in his observations made at

dipping-needle in the same years. All the iron braces were on the north-west side of the needle, and the nearest about 18 feet from it.

The allowances made to the observations of the variation, and also of the dip, for the effect of the iron-work of the room, were both ascertained after the above-mentioned alteration in the iron-work took place; but they have, notwithstanding, been applied to the observations made before, as well as since that time.

Remarks, &c. at the Royal Observatory at Paris, between the years 1783 and 1788, but the effect was considerably greater in his observations, than in those mentioned above; his results however were, in my opinion, drawn from too few observations, being from only 8 days observations about the times of the equinoxes and solstices, which differ considerably among themselves; and experience teaches us, that magnetical observations, made for a period so limited, are not sufficient for minute purposes: I have, therefore, in the results here given, taken the mean of the observations made during the whole month in which the equinoxes and solstices fall, which appear to me likely to furnish results more satisfactory; and all the foregoing observations are to be considered as the results or mean of a great many, by way of arriving at greater accuracy than could be obtained without; this, however, was found to be more necessary at some times than at others; sometimes the needle would be extremely consistent with itself, so as to return exactly to the same point, however often it might have been drawn aside; at other times it varied 2 or 3′, sometimes 8, 10′, or even more: this uncertainty in the needle arises principally, I believe, from changes in the atmosphere, for a change of wind, from any quarter to another, almost always produced a change in the needle from steady to unsteady, and *vice versâ*, but it was generally more unsteady with an easterly wind than when it blew from any other quarter, and most steady when the wind was south or south-westerly. An Aurora Borealis always produced considerable agitation of the needle.

Unsteadiness of the needle.

Annual increase of the variation.

It has been mentioned in this Paper, that the annual increase of variation was found about the year 1775 to be nearly 10′; and was considered at that time to be gradually diminishing; but it is remarkable that this rate of increase appears, from the annexed Table, to be nearly the same at which it has been found to move between all the different periods in the said Table, from 1580 to 1787, a period of more than 200 years, excepting between the years 1692 and 1723: the observations of Halley in 1692, and Mr. Graham in 1723, make the annual increase 16′: to what this difference could be owing I am at a loss to account: on referring to observations made at Paris for those two years, the annual increase is 14′; subsequent observations made by Mr. Graham

ham in 1748, make the annual increase between this year and 1723 only 8',1 nearly what its rate had been found before this great difference occurred; and from the variation of Mr. Graham in 1748, and the variation observed by Dr. Heberden in 1773, the annual increase is 8',4; the variation in 1773, compared with the variation observed by myself in 1787, give for the annual rate of increase 9',3; but between 1787 and 1795, the annual increase was only 4',7; between 1795 and 1102, 1',2; and between 1802 and 1809, only 0',7.

The mean rate of annual increase for the above mentioned period of 207 years, viz. from 1580 to 1787, is 10'.

Changes in the variation for 200 years.

As there appears something curious in the rate at which the variation has been moving, from observations made at London, for a period of more than 200 years, the annual increase of which during that time continued nearly the same; but in a subsequent period of 18 years only, the decrease of that annual increase became so rapid, that the annual increase in the latter part of it does not amount to quite one minute, I shall subjoin the following Table, by way of elucidating what is here mentioned.

Table of variation for 225 years at London.

By whom the Variation was observed	Year.	Variation. Annual	Increase.
		° ′	′
Mr. Burrows*	1580	11 15 E	
Mr. Gunter	1622	6 0	+ 7,5
Mr. Gellibrand	1634	4 6	9,6
Mr. Bond†	1657	0 0	10,6
Mr. Gellibrand‡	1665	1 22W	10,2
Dr. Halley§	1672	2 30	9,7
———	1692	6 0	10,5
Mr. Graham‖	1723	14 17	16,0
———	1748	17 40	8,1
Dr. Heberden¶	1773	21 9	8,4
Mr. Gilpin	1787	23 19	9,3
———	1795	23 57	4,7
———	1802	24 6	1,2
———	1805	24 8	0,7

* The observations of Burrows, Gunter, and Gellibrand, in 1634, are taken from Seller's Practical Navigation, 1676. Burrow's observations are said to be the oldest and best in the world; longitude and latitude found by dipping-needle, p. xvi. Gellibrand is said to be the first person who ascertained the variation of the variation, about the year 1695, Phil. Trans. No. 276—278; but if this is the date of the observations by which it was determined, the observations of Gunter, in 1662, show him to have a prior claim; Bond, in his Longitude Found, p. 5 and 6, says, that the variation was first found to decrease by Mr. John Mair, secondly by Mr. Edmund Gunter, thirdly by Mr. Henry Gellibrand, and by himself, in 1640.

† Longitude Found, p. 3.

‡ Ibid. p. 13; and Longitude and Latitude found by Dipping-Needle, p. 6.

§ Phil. Trans. No. 195, p. 565.

‖ Ibid. No. 383, p. 107; and No. 488, p. 279.

¶ Obligingly communicated by his son, the present Dr. Heberden.

Table

Table V. contains the dip of the magnetic needle from the years 1786 to 1805. For the first sixteen months, viz. from September, 1786, to December, 1787, both inclusive, the dip was observed as freqnently as the variation, *but as there does not appear to be any diurnal alteration in the dip,* to make it at all interesting to communicate so many observations as were made, the mean therefore for each month has been thought sufficient for insertion.

Dip of the needle from 1786 to 1807. No diurnal change.

To explain the foregoing Table it must be observed, that each of the numbers in the four first columns of the above Table, are each of them the mean of several means, as expressed in the line against those numbers; and as each of those means, are again the mean of five observations at least, each of the numbers in the first line, said to be the mean of nine means, is therefore a mean of forty-five observations; and so of all the rest.

The numbers in the fifth column, entitled true dip, are the means of the numbers contained in the four preceding columns in the same line with it.

The dipping needles used by Norman, the inventor of the dipping needle, who observed the dip at London in the year 1576 to be * 71° 50'; and of Mr. Bond, who observed it in 1676 to be † 73° 47'; not being so much to be depended upon as the needles that have been in use for near a centnry past, render the progressive increase of the dip from Norman's time, to the time of its maximum, somewhat doubtful. But Mr. Whiston, whose needle there is reason to believe was more to be relied upon, in the year 1720 determined the dip to be ‡ 75° 10'; this, when compared with many, and very accurate observations made by Mr. Cavendish with several needles, in the year ‖ 1775, who found it to be 72° 30', makes the decrease in this period, of 55 years on a mean, 2' 9 *per annum.* And from a comparison of my own observations of the dip in 1805, which was 70° 21', with the above of Mr. Cavendish in 1775, its annual decrease, on a mean, appears to have been 4', 3; and its progressive annual decrease, on a mean, in the above mentioned period of 30 years, 1', 4.

The dip seems to have formerly encreased; but for the last century has increased.

I can-

* New Attractive, c. 4. † Longitude found.

‡ Longitude and latitude found by dipping needle, p. 7—94.

‖ Phil. Trans. Vol. LXVI, p. 400.

It is much to be regretted that observations of variations have not been oftener made.

I cannot conclude this Paper without expressing my regret, that so little avail should have been made of the numerous opportunities which have been afforded to travellors and others, in the last century, for making accurate observations, with proper instruments, at land, on the variation in different parts of the world. Such observations would probably have afforded some curious and useful facts, which would have materially assisted in forming a theory much more certain than what we at present possess; the present received opinion of the cause of the diurnal alteration of variation would be confirmed or invalidated; its quantity of effect in different places, a most desirable acquisition, would be ascertained; and we should be put in possession of more valuable and correct information on the variation than can be derived from observations made with the common azimuth compass, even at land, owing to its imperfect construction. The variation thus accurately obtained at any one period, compared with the variation correctly ascertained at a subsequent period, would give a rate of alteration of the variation which could be relied on.

Obcrvations and researches of Halley.

The celebrated Halley thought the variation of so much importance, that he made two voyages for the purpose of making observations on the variation, to confirm his theory advanced in 1613, and soon after he published his variation chart. Since his time no better theory than he left has been obtained, although it must be confessed that many observations have been made at sea by voyagers; but these observations, made generally to answer the purpose of the observer at the time only, are therefore seldom preserved; for, unless made by authority, which rarely happens, they do not often meet the public eye; and it must be from observations made with care, and with good instruments, carefully registered, and properly arranged, that any real advantage can be derived. It is hoped therefore, that, in future, attention to this subject will not be thought beneath those who may have it in their power essentially to promote an undertaking so interesting to the philosopher, and so valuable and useful to the maritime world.

TABLE

TABLE II.—*Mean monthly Variation of the magnetic Needle.*

1786	6 A.M.	7 A.M.	8 A.M.	10 A.M.	12 M.	1 P.M.	2 P.M.	4 P.M.	6 P.M.	8 P.M.	10 P.M.	11 P.M.
	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′
Sept.	23 —	23 7,9	23 10,1	23 14,5	23 22,2	23 23,7	23 23,9	23 19,0	23 15,3	23 13,5	23 12,4	23 —
Oct.	—	10,4	11,3	15,2	24,4	26,1	26,1	21,1	17,7	15,6	14,5	13,8
Nov.	—	12,2	12,5	15,3	21,6	22,5	22,0	20,3	17,6	15,9	15,1	14,7
Dec.	—	—	14,5	16,1	20,6	22,0	22,2	20,0	17,4	15,8	15,0	15,0
1787												
Jan.	—	14,0	14,2	17,1	22,3	24,1	24,5	21,8	18,4	15,6	14,5	14,8
Feb.	—	14,2	15,1	17,1	23,3	24,8	25,1	23,7	18,8	15,3	15,8	12,8
Mar.	—	12,8	12,8	15,3	26,5	27,7	27,8	18,4	19,0	15,9	15,5	15,7
April	9,7	9,9	9,7	13,9	23,6	27,0	27,4	22,6	17,8	15,7	15,7	15,6
May	7,6	7,5	7,4	13,5	25,2	26,6	26,2	21,0	17,7	17,1	16,8	17,0
June	8,4	8,2	8,8	16,0	26,6	28,1	28,1	22,6	18,7	17,9	17,8	17,7
July	9,5	9,6	10,3	17,8	27,6	29,3	29,4	23,2	19,4	18,9	19,3	19,1
Aug.	11,9	12,0	12,8	19,7	30,3	31,7	31,5	25,6	19,3	18,7	18,9	18,8
Sept.	15,0	15,1	15,3	20,2	29,8	30,7	30,5	24,7	20,1	19,1	19,2	19,2
Oct.	—	17,5	17,3	21,1	30,8	31,9	31,5	27,4	21,9	20,8	20,2	19,6
Nov.	—	19,4	19,7	20,6	29,7	31,1	30,2	27,7	22,7	21,4	21,3	21,4
Dec.	—	20,4	21,0	21,8	28,2	29,0	29,0	26,2	22,9	21,9	21,6	—

TABLE III.—*Mean monthly true Variation, and mean monthly diurnal Alteration of Variation of the magnetic Needle.*

	January.		February.		March.		April.		May.		June.	
	True Variation.	Diurnal Alteration of Variation.	True Variation.	Diurnal Alteration of Variation.	True Variation.	Diurnal Alteration of Variation.	True Variation.	Diurnal Alteration of Variation.	True Variation.	Diurnal Alteration of Variation.	True Variation.	Diurnal Alteration of Variation.
	° ′	′	° ′	′	° ′	′	° ′	′	° ′	′	° ′	′
1786	—	—	—	—	—	—	—	—	—	—	—	—
1787	23 19,2	10,2	23 19,8	10,4	23 20,3	15,0	23 18,5	17,4	23 17,0	18,9	23 18,3	19,6
1788	23 25,6	8,7	—	—	—	—	—	—	—	—	23 38,9	18,8
1789	—	—	—	—	—	—	—	—	—	—	23 34,2	17,1
1790	23 38,9	8,4	—	—	—	—	—	—	—	—	—	—
1791	23 35,6	6,8	—	—	—	—	23 36,0	15,0	—	—	—	—
1792	23 41,1	5,4	—	—	—	—	—	—	23 41,9	11,5	—	—
1793	23 46,9	4,3	23 48,3	4,6	23 48,8	8,5	23 46,2	11,7	23 47,3	10,1	23 48,5	12,6
1794	23 54,2	4,5	—	—	—	—	—	—	—	—	—	—
1795	—	—	—	—	23 57,5	9,8	—	—	—	—	23 57,1	9,4
1796	—	—	—	—	24 1,1	7,0	—	—	—	—	23 58,7	9,8
1797	—	—	—	—	24 1,5	7,4	—	—	—	—	24 0,2	11,6
1798	—	—	—	—	24 0,6	7,2	—	—	—	—	23 59,4	11,2
1799	—	—	—	—	24 1,1	7,5	—	—	—	—	24 0,6	10,8
1800	—	—	—	—	24 3,6	6,9	—	—	—	—	24 1,8	10,9
1801	—	—	—	—	24 5,2	8,8	—	—	—	—	24 2,8	10,8
1802	—	—	—	—	24 6,9	9,5	—	—	—	—	24 5,[illegible]	10,7
1803	—	—	—	—	24 8,0	11,8	—	—	—	—	24 7,0	12,6
1804	—	—	—	—	24 9,4	10,0	—	—	—	—	24 6,0	11,3
1805	—	—	—	—	24 8,7	8,1	—	—	—	—	24 7,8	12,5

TABLE

TABLE III.—*Mean monthly true Variation, and mean monthly diurnal Alteration of Variation of the magnetic Needle.*

	July		August.		September.		October.		November.		December.	
	True Variation.	Diurnal Alteration of Variation.	True Variation.	Diurnal Alteration of Variation.	True Variation.	Diurnal Alteration of Variation.	True Variation.	Diurnal Alteration of Variation.	True Variation.	Diurnal Alteration of Variation.	True Variation.	Diurnal Alteration of Variation.
	° ′	′	° ′	′	° ′	′	° ′	′	° ′	′	° ′	′
1786	—	—	—	—	23 16,4	14,8	23 18,4	15,3	23 17,3	9,9	23 18,3	7,6
1787	23 19,6	19,6	23 21,9	19,4	23 22,8	15,5	23 24,5	14,3	23 25,0	11,1	23 25,8	8,3
1788	23 29,6	16,4	—	—	—	—	23 32,1	14,6	—	—	—	—
1789	—	—	—	—	—	—	—	—	—	—	23 41,2	5,4
1790	23 39,0	15,4	—	—	—	—	—	—	—	—	—	—
1791	23 38,7	15,2	—	—	—	—	—	—	—	—	—	—
1792	—	—	23 43,6	12,7	23 43,9	11,1	23 45,6	8,9	23 45,9	3,7	23 45,2	3,1
1793	23 50,5	12,5	23 48,6	12,1	23 52 6	9,8	23 52,3	7,0	23 51,9	3,8	23 52,3	3,8
1794	23 54,4	11,2	23 57,2	9,8	23 58,1	8,4	—	—	—	—	—	—
1795	23 57,1	8,4	—	—	24 0,4	7,6	—	—	—	—	23 59,4	3,6
1796	23 69,2	10,1	—	—	24 0,1	8,3	—	—	—	—	24 1,3	4,9
1797	24 0,3	10,1	—	—	24 1,4	7,6	—	—	—	—	24 1,3	5,0
1798	24 0,0	10,0	—	—	24 1,4	9,4	—	—	—	—	24 1,4	2,7
1799	24 1,8	10,4	—	—	24 2,9	7,8	—	—	—	—	24 2,3	3,4
1800	24 3,0	9,2	—	—	24 3,6	7,7	—	—	—	—	24 3,3	3,1
1801	24 4,1	10,3	—	—	24 3,8	10,1	—	—	—	—	24 5,4	2,5
1802	24 6,0	12,3	—	—	24 8,7	8,9	—	—	—	—	24 6,8	3,8
1803	24 7,9	13,1	—	—	24 10,5	9,5	—	—	—	—	24 10,7	3,0
1804	24 8,4	10,4	—	—	24 8,9	9,3	—	—	—	—	24 9,0	3,7
1805	24 7,8	10,4	—	—	24 10,0	9,3	—	—	—	—	24 9,4	4,0

TABLE IV.—*Differences between the Observations of the Variation of the magnetic Needle, at the times of the Equinoxes and those of the Solstices.*

Years.	March.	June.	September	December.
1793	+ 3,6	— 0,3	+ 4,1	— 0,3
1795	——	— 0,4	+ 3,3	— 1,0
1796	+ 7	— 2,4	+ 1,4	+ 1,2
1797	+ 0,2	— 1,3	+ 1,2	— 0,1
1798	— 0,7	— 1,2	+ 2,0	0,0
1799	— 0,3	— 0,5	+ 2,3	— 0,6
1800	+ 1,3	— 1,8	+ 1,8	— 0,3
1801	+ 1,9	— 2,4	+ 1,0	+ 1,6
1802	+ 1,5	— 1,6	+ 3,4	— 1,9
1803	+ 1.2	— 1,0	+ 3,5	+ 0,2
1804	— 1.3	— 3,4	+ 2,9	+ 0,1
1805	— 0.3	— 0,9	+ 2,2	— 0,6
Mean	+0′.80	—1′,48	+ 2′,43	—0′,14

VII.

A few Remarks on a Pamphlet entitled "Mr. W. Nicholson's "Attack, in his Philosophical Journal, on Mr. Winsor and "the National Light and Heat Company, with Mr. Winsor's "Defence"—12mo. 56 pages.

Mr. Winsor's pamphlet lately published.

MR. Winsor seems to have overlooked the observations in our Journal for January last, as long as prudence and the inquiries of his visitors would allow him to follow that mode of conduct. After having answered a question of public importance openly and without reserve, in my own name, as every man ought, where the character of another can in any respect be brought in question, I may be allowed to decline all controversy, and leave Mr. Winsor's claims upon me and upon the world,

world, to be settled from the facts as they stand. I think it would be extremely easy to shew the numerous, and, in some instances voluntary, errors and misrepresentations with which his pamphlet is vitiated; but I am called upon by no duty to do this.

To my readers, it is, I trust, needless to repeat the truths I have already laid down. I can have no cause of enmity to Mr. Winsor; but I am not indifferent to the question, whether the public shall be deluded, when one of that public asks me to give an opinion upon what every man has a right to examine. It is this motive which leads me at present to notice his pamphlet, and induces me not to dismiss the subject without a few general observations. Reasons why the Editor notices it.

It certainly is Mr. Winsor's duty to give all those means of satisfaction in his printed proposals, which are usually tendered when undertakings of credit are offered for public support. The following remarks will shew that he has not done this: Mr. Winsor has not given satisfaction to the public.

1. He asserts, that a public committee was appointed to verify his discovery. He ought to have said who appointed them, where are their minutes, and *what are their names.* His pretended committee is unknown,

2. He pretends that his patent is vested in four respectable gentlemen, as co-proprietors, who propose to establish a company. *He ought to have published their names.* —and also the proprietors who propose the company.

3. On behalf of these four concealed gentlemen and himself, he asks for subscriptions; of which the proposed deposits amount to one hundred thousand pounds, by twenty thousand subscribers; and there is to be no meeting of the intended company till one fourth part of this sum (twenty-five thousand pounds) has been deposited. Mr. Winsor either has an uncontrouled power to draw this £20,000 from the banking-houses, or he has not. If he has not, the sum is in trust either with the four co-proprietors, or with other nominees, or with the bankers. In any or either case, the trust ought to have been declared and published; and the trustees themselves, by name, would in fact stand pledged for the honour and credit of the whole project. Either Mr. Winsor holds the first £20,000, or the holders are concealed.

4. If Mr. Winsor, in his pamphlet, instead of running into a long and faulty dissertation on the law of patents, had laid my remarks (since the dignity he asserts has not forbad him to notice them) before counsel; or if he had asked the simple question of any eminent legal man—" Whether the granting of Mr. Winsor ought to establish his patent, by offering a plain question to counsel,

 " licences,

" licences, to exercise any part of a patent privilege, to a " number exceeding five, *limited by covenant*, be not such a " sharin f the monopoly as will render the patent void?"— He would have shown at once how far I may have presumed to speak positively where it became me to doubt; and he might have set the minds of his subscribers at rest upon that point. I have asserted, that if he has disposed of such licences, which he very properly calls *sharing the privileges*, to a thousand persons out of a limited twenty thousand, as he asserts, he has annulled his patent. He will not find a lawyer who will maintain the contrary.

I am persuaded that the magnitude, as well as the philosophical nature of the subject, and the interest which so large a part of the manufacturers of the British empire have in patent rights, will render the preceding observations of sufficient importance to require no apology.

VIII.

Account of the Small Whales in the Seas near the Shetland Isles. By PATRICK NEILL, *A. M. Secretary to the Natural History Society at Edinburgh.**

Shoal of small whales.

BY a letter from a gentleman at Uvea Sound, Unst, I was informed, that, " on the 21st February, 1805, no fewer than 190 small whales, from six to twenty feet long, were forced ashore at Uvea Sound; and on the 19th March thereafter, 120 more at the same spot, in all, 310. In this second shoal there were about 500, but very many escaped." To a series of queries addressed to the same gentleman, I received in substance the following answers. " They measured from six to twenty-four feet in length: the small ones appeared to be the young of the others. They had two long and narrow pectoral fins, from between four and five feet to even nine feet long. They remained at the surface of the water ten or fifteen minutes

* From his Tour to the islands of Orkney and Shetland.

nutes, just as the boats were near or distant. They had one small fin on the back. The people called them *bottle-noses*, and *common black whales*, but most generally *ca'ing whales*. They had a row of teeth, $1\frac{1}{2}$ inches long, in both jaws, about two dozen in number in each jaw. The upper jaw was rather the widest. They had no whalebone in the mouth, and had only one blow-hole, situated in a small hollow at the back of the head. Most of the females were either with young or giving suck. Many of the young ones had got no teeth. They had all very fine black skins, as soft and smooth as silk. They appeared to be very inoffensive animals, and shewed much natural affection for each other: when any one first struck the ground, it set up a kind of howling cry, and immediately others crowded to the spot, as for its relief. *Sandy giddocks* (sand-lances) were found in their mouths." From information furnished by another gentleman, I further learned, that "from the tip of the nose to the last vertebra of the back-bone, the generality of the whales measured twenty feet: that the head was short and round, resembling in shape the head of a seal; and the upper jaw projected three or four inches over the lower."—Numbers of the females (this gentleman adds) were suckling their young when driven ashore; and while they continued alive, the milk was seen to issue from their nipples: of these they had only two, resembling the teats of a cow, but larger."

Account of the small whales which frequent the north coast of Scotland, &c.

This kind of whale sometimes appears, in large herds, off the Orkney, and especially the Shetland islands. Being of a gregarious disposition, the main body of the drove follows the leading whales, as a flock of sheep follows the wedders. Hence the name of *ca'ing whales*, bestowed on them by the natives, who well know that if they are able to guide the leaders into a bay, they are sure of likewise entangling multitudes of their followers. Though the above description proves that they belong to the genus Delphinus, and are nearly allied to the Delphinus Orca or Grampus, they appear to me to differ in several respects from that, or any of the other species described by naturalists, so much at least, as to deserve the attention of gentlemen who may hereafter enjoy opportunities of accurate observation. I shall briefly enumerate the points of dissimilarity.

Account of the small whales which frequent the north coast of Scotland, &c.

The grampus has the snout "spreading upwards," according to Shaw*; "waved upwards," according to Stewart†; "*sursùm repando*," as Linnæus expresses it. But this character was not to be found in the *ca'ing whales*, in which the nose was neither spread nor turned up at the end, but rounded and dropping. But I must remark, that La Cepède (the able continuator of Buffon's "Histoire Naturelle," and whose general accuracy is great) takes no notice whatever of the "waving or spreading upwards," the "sursùm repando," mentioned by preceding authors.

In the grampus, according to Shaw, "the lower jaw is much wider than the upper," in the ca'ing whale: however, we find that "the upper jaw is the widest."

The grampus is said, in books, to have thirty teeth in each jaw: the Uvea-Sound whales had only twenty-four in each jaw. But La Cepède remarks, that the number of visible teeth varies with the age of the animal.

In Dr. Shaw's figure of the grampus (which, I must confess, is inferior in accuracy to that of La Cepède), the pectoral fins are short and round; according to La Cepède, they are "*larges et presque ovales*‡." In the ca'ing whale they are said to be long and narrow,"—thus bearing more resemblance to those of the Delphinus gladiator (to be afterwards spoken of).

"The back fin," says Dr. Shaw, "measures six feet in height." In the largest of the Uvea whales it did not exceed two feet. La Cepède does not make it so long as Shaw.

The eye of the ca'ing whale, I am informed, was placed higher in the head than in Shaw's figure; and the spiracle, as we have seen, was "situated in a small hollow at the back of the head," and behind the eye: no such hollow is delineated in Dr. Shaw's plate; but this is probably an oversight, as it is distinctly depicted in La Cepède's representation of the same animal.

The Uvea whales had not the white spot on each shoulder, near the eye, described as appearing in the grampus, and figured

* "General Zoology," *in loco.*

† "Elements of Natural History," 2 vols. 8vo.

‡ "Histoire Naturelle des Cétacées, par le citoyen La Cepède," p. 301, 4to. Paris, l'an xii.

figured by Shaw. But La Cepède only says, "On voit *souvent* derrière l'œil une grande tache blanche.*"

Account of the small whales which frequent the north coast of Scotland, &c.

The neck, breast, and belly were not, I am told, white, as in the grampus, nor was there a defined line between the dark and light parts. Some of the ca'ing whales were, according to my information, quite black; others, especially females, had only a little grey on the belly.

The grampus, we are told†, "seldom remains a moment above water:" the Uvea whales, however, as formerly observed, "remained ten or fifteen minutes at the surface, just as the boats were near or distant."

The grampus is stated by Dr. Shaw to be a "very ferocious animal, attacking seals and porpusses:" it has long been considered as the formidable *sea-monster* spoken of by the ancients‡: but the *ca'ing whale* appeared to be a very inoffensive animal, and the common sand-lance was observed to be its food.

Under the name of *grampus*, a similar animal, called by La Cepède, *le Dauphin gladiateur*, has generally been confounded. The dorsal fin, however, stands much higher than in the grampus, and nearer to the head. The pectoral fin is long and narrow like an oar. It is this species, and not the common grampus, that attacks whales, fastening around them like so many bull-dogs, and making them bellow with pain: hence sailors call it the *killer*. One of this species was, in 1793, taken in the Thames; a drawing and description of which appears

* "Histoire des Cétacées," &c. p. 300.

† Bingley's "Natural Biography," vol. ii, p. 152.

‡ The *small-eyed cachalot* (Physeter microps) must certainly be a much more terrible-looking animal. Its head is very large, forming indeed nearly one half of the whole body, which is from 40 to 60 feet long. It is known to be very ferocious, having been seen to attack and tear to pieces the huge Greenland whale. It is not without reason, therefore, that La Cepède rather considers this animal as the *sea-monster* of the ancient mythologists—from the devouring jaws of which Perseus delivered the fair candidate for the prize of beauty (Andromeda), and the horrific aspect of which struck terror into the fiery steeds of Hippolytus. It was a cachalot of this kind that was, in the end of the year 1769, stranded at Cramond, near Edinburgh, and which attracted many thousands of spectators from that city.—Stark's Picture of Edinburgh, p. 465.

Account of the small whales which frequent the north coast of Scotland, &c.

pears to have been sent by Sir Joseph Banks to La Cepède, who has figured it in his "Histoire des Cétacées."

The small whales in question, of whatever species they be, afford a great deal of blubber; and it appears surprising that the value of the oil does not induce some of the Shetland and Orkney gentlemen or some of the few substantial tenants, to prepare and keep in readiness an ample store of harpoons, ropes, whale-lances, blubber-knives, and other implements, so as to enable their dependants to avail themselves, more completely than is at present possible, of the occasional visits of those cetaceous inhabitants of the northern seas. Harpoons and lines are indispensably necessary. The best harpoons, I believe, may be commissioned from Prestonpans, at the rate of 7s. 6d. each. A single line for each harpoon would suffice, and that line needs not be of the thickness required for Greenland whales: the Greenland whale-lines cost 5l. but a line sufficient for the small whales might be had for 2l. sterling. Each boat might carry six harpoons and lines, provided only care were taken to keep the lines clear of each other. Each man should be furnished with a *lance*, *i. e.* a kind of spear with a wooden handle six feet long, costing 5s. each. Blubber-knives may be had at 2s. 6d. each. The hooked instrument called *tomakawk* or *pickihawk*, is also very useful for laying hold of the blubber, and keeping it on the stretch till it be cut. If the blubber is to be barrelled, it should be allowed to lie exposed to the air for a day or two, till incipient putrefaction be perceived; for the swelling that accompanies the commencement of that process would infallibly burst the barrels It is scarce necessary to add, that a large caldron would be found very useful for boiling down the blubber.

The exertions of the Shetland tenants, with respect to such droves of small whales, must certainly be much cramped by the usage of the country, which I have now to relate, and which appears to me equally destitute of foundation in law and in equity. I shall state the usage in the words of Mr. Giffard of Busta, which are certainly above all exception: "As soon as the whales are got ashore (*i. e.* by the exertions of the people, who,

who, surrounding them with boats, embay them, and force them ashore), the bailie of the parish is advertised, who comes to the place, and takes care that none of them are embezzled; and he acquaints the Admiral thereof, who forthwith goes there, and holds a court, where the fiscal presents a petition, reciting the number of whales, &c. that the judge may give judgment thereupon, according to law and the country practice. Whereupon the Admiral ordains the whales driven ashore to be divided in three equal parts; one to belong to himself; one to the salvers; and the third to the proprietor of the ground on which the whales are driven ashore*." It is added, that the minister of the parish demands tithes of them, and that the bailie of the parish claims the head as a perquisite. Mr. Giffard fortunately informs us, that the "biggest" of the whales of which he is speaking, "are from eighteen to twenty feet long."

Account of the small whales which frequent the north coast of Scotland, &c.

Let us now examine how the law stands on this subject. "By the *leges forestarum*, § 17 (says Mr. Erskine), all *great* whales belong to the King, and all such smaller whales as may not be drawn from the water to the nearest part of the land on a wain with six oxen. But no whales have, for at least half a century past, been claimed, either by the King, or by the Admiral his donatory, but such as were of a size considerably larger than there described."

IX.

* Account of Zealand, by Thomas Giffard of Busta, 1733, in *Bibliotheca Britannica topographica*, No. 38.

† Institute, b. ii, tit. 1, § 10.

IX.

Method of preparing Pannels for Painters. By Mr. S. Grandi.*

Composition for grounding pannels. It is made of calcined bones and wheat flour.

TAKE the bones of sheep's trotters, break them grossly, and boil them in water until cleared from their grease, then put them into a crucible, calcine them, and afterwards grind them to powder. Take some wheaten flour, put it in a pan over a slow fire until it is dry, then make it into a thin paste, add an equal quantity of the powdered bone-ash, and grind the whole mass well together: this mixture forms the ground for the pannel.

Application.

The pannel having been previously pumiced, some of the mixture above-mentioned is rubbed well thereon with a pumice-stone, to incorporate it with the pannel. Another coat of the composition is then applied with a brush upon the pannel, and suffered to dry, and the surface afterwards rubbed over with sand-paper.

A thin coat of the composition is then applied with a brush, and if a coloured ground is wanted, one or two coats of the colour is added, so as to complete the absorbent ground.

When it is necessary to paint upon a pannel thus prepared, it must be rubbed over with a coat of raw linseed or poppy-oil, as drying oil would destroy the absorbent quality of the ground; and the painter's colours should be mixed up with the purified oil hereafter mentioned.

Canvas grounds.

Canvass grounds are prepared, by giving them a thin coat of the composition, afterwards drying and pumicing them, then giving them a second coat, and, lastly, a coat of colouring matter along with the composition.

The grounds thus prepared do not crack; they may be painted upon a very short time after being laid, and from their absorbent quality, allow the business to be proceeded upon with greater facility and better effect than with those prepared in the usual mode.

Method

* The processes of Mr. Grandi being founded upon practice, were supported to the Society of Arts, by certificates from our most eminent painters; in consequence of which, and of the exhibition of the Pannels, the Society awarded him the Silver Medal and 20 guineas.

Method of purifying Oil for Painting. Make some of the bone-ashes into a paste with a little water, so as to form a mass or ball; put this ball into the fire, and make it red hot; then immerse it for an hour, in a quantity of raw linseed oil, sufficient to cover it: when cold, pour the oil into bottles, add to it a little bone-ash, let it stand to settle, and in a day it will be clear and fit for use. Purifying oil.

White Colour is made by calcining the bone of sheep's trotters in a clear open fire, till they become a perfect white, which will never change. White colour.

Brown Colour is made from bones in a similar manner, only calcining them in a crucible instead of an open fire. Brown.

Yellow-Colour, or Masticot. Take a piece of soft brick, of a yellowish colour, and burn it in the fire; then take for every pound of brick, a quarter of a pound of flake-white, grind them together and calcine them; afterwards wash the mixture, to separate the sand, and let the finer part gradually dry for use. Yellow.

Red-Colour, equal to Indian-Red. Take some of the pyrites, usually found in coal-pits, calcine them, and they will produce a beautiful red Red.

Grey Colour is made by calcining together blue-slate and bone-ashes powdered, grinding them together, afterwards washing them, and drying the mixture gradually. Grey.

Blue-Black is made by burning vine-stalks in a close crucible in a slow fire, till a perfect charcoal is made of them, which must be well ground for use. Blue-black.

Crayons are made of bone-ash powder mixed with spermaceti, adding thereto the colouring matters. The proper proportion is, three ounces of spermaceti to one pound of the powder. The spermaceti to be first diffused in a pint of boiling water, then the white bone-ash added, and the whole to be well ground together, with as much of the colouring matter as may be necessary for the shade of colour wanted. They are then to be rolled up in the proper form, and gradually dried upon a board. Crayons.

White Chalk, if required to work soft, is made by adding a quarter of a pound of whitening to one pound of the bone-ash powder will answer alone. The coloured chalks are made by grinding the colouring matter with bone-ashes. White chalk.

SCIENTIFIC NEWS,

AND OTHER MISCELLANEOUS ARTICLES.

Small portable Fire Engine.

MR. HORBLOWER, of Featherstone Street, City Road, with whose talents the world is well acquainted, has requested me to mention a construction of the fire engine which he has made, which renders it of much utility within the apartments of an house. I have not seen the engine; but he states, that it stands in the compass of fourteen inches square and two feet high, and may be carried from one room to another with ease. He finds, by experiment, that the four sides of a bed-room, all on fire, may be extinguished in the space of a minute, by little more than a pail of water. All that is required is to keep it filled in its proper place, and to work it off every month or six weeks, for the purpose of changing the water and ascertaining that it is in proper working state.

Enquiry respecting Grease Spots.

A Correspondent requests to be informed of a method of discharging grease spots from coloured goods, composed of silk and worsted. He observes, that it frequently happens, in the process of weaving, that the tallow or oil drops on the work from the candles or lamps used by the weavers, and forms spots which render the goods quite unsaleable; and he suggests, that if any of the readers of this Journal should point out a method of discharging them, they would render a considerable service to manufacturers.

I wish it were in my power to point out the remedy here desired from actual experiment; but I must leave the answer to others, and shall only venture to speak in general terms of the

the means by which spots of grease are usually taken out of piece goods. These methods are reducible to two; namely, absorption and ablution. When an absorbent earth (fullers earth or tobacco pipe-clay for example), is applied wet upon a place which is greased, the oil usually flows into the capillary interstices of the earth, as the water evaporates; and, upon beating or rubbing out the dry earth, the vegetable or animal fibre is left clean. When the oil is solid, at the common temperature, as is the case with tallow or wax, it is found necessary to apply the heat of an iron or common fire cautiously to the place, while the earth is drying. In some description of goods blotting paper, or bran, or raw starch, may be used with advantage. In these manipulations the difficulty of taking out the grease does not seem to be so great as that of avoiding injury to the face of the goods.

The method of taking out grease by ablution is perfectly well known. Water acts upon grease by the medium of soap, or less safely by the interposition of an alkali. The chemical action of these, as well as the probability of mischief from applying water to various descriptions of goods, oppose insurmountable obstacles to their use in many instances. I have not tried how far the solution of pure ammonia might be beneficial in processes of this kind. It promises the advantage of quitting the article by evaporation, after the process is over. There is a method, commonly used for taking grease spots out of silks, which may probably be intitled to further extension. Alcohol, or spirit of wine, does not act upon grease or fat oil by itself; but when the volatile oil of lemons, called essence of lemons, is dissolved in that fluid, the compound will take out grease spots. The method of applying it is to wet the place, and wipe or rub it while wet with a sponge or cloth. It might be worth trying whether a much cheaper essential or volatile oil than that of lemons might not be used for this purpose. Spirit of turpentine would have an unpleasant smell for a time; but, perhaps, it would not last.

Dr. Clanny, of Durham, has just published an history and analysis of the mineral waters at Butterby, near that town.

I can-

TO CORRESPONDENTS.

It would be an unpleasant, as well as a difficult task, for me to state the reasons which may at any time require me, as Editor of the Philosophical Journal, to decline inserting some of the papers which may be sent to me. It is obvious that a variety of very proper inducements may offer themselves to govern my conduct in that respect, which it would answer no useful purpose to detail. My Correspondents have accordingly, in almost every instance, received this information in private, where it has been required; but, for the most part, I have been allowed to excuse my discretion without enquiry. Among a few papers which remain with me, and are not intended for insertion, one from H. B. K. has been the subject of enquiry and remonstrance from the writer. As he is unknown, I have no other than the present channel to say, that his Paper will be returned to the bearer of an order, in the same hand writing; and as he complains of a want of justice in its not having been inserted, I must remark, that though his discussions appeared to me to have become too extensive for monthly insertion, I should, nevertheless, have admitted that paper, if I had not thought that the spirit of controversy between himself and Mr. Sylvester was becoming too personal to be interesting to the readers of this Journal.

I cannot at present answer the enquiries of R. P. respecting the application of muriatic acid to promote vegetation; but I will satisfy myself whether the alledged facts on that behalf may be entitled to attention.

Errata in Dr. Bostock's Paper on Palm Oil.

Page 163, l. 5, *for* alexginons, *read* oleaginous.
l. 9, *for* flashes, *read* flakes.
l. 13, *after* "in" *insert* "the."
Page 164, l. 3, from bottom, *for* resin, *read* resins.
Page 166, l. 17, from bottom, *for* 52 gs. *read* .52 gr.
l. 15, from bottom. *for* 1-8th gr. *read* .8 gr.

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A

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NATURAL PHILOSOPHY, CHEMISTRY

AND

THE ARTS.

MAY, 1807.

ARTICLE I.

Description of a New Astrometer for finding the Rising and Setting of the Stars and Planets, and their Position in the Heavens. *By* DAVID BREWSTER, *A. M.*

To Mr. NICHOLSON,

SIR,

AN astrometer for finding the rising and setting of the stars, was invented about thirty years ago, by M. Jeurat, of the Academy of Sciences at Paris, and is described in the Memoirs of that learned body. The utility of this instrument in abridging the computation of semidiurnal arcs, where great precision is unnecessary, renders it highly interesting to those who are engaged in the study or practice of astronomy, and has induced me to send you the description of a new astrometer, more simple in its construction, and more extensive in its application, than that invented by M. Jeurat.

Description of an astrometer for determining the apparent situation of the stars, &c

This astrometer, represented in Plate II. Fig. 1, consists of four divided circumferences. The innermost of these is moveable round the center A, and is divided into twenty-four hours, which are again subdivided into quarters and minutes, when the circle is sufficiently large. The second circumference is composed of four quadrants of declination, divided by means of a table of semidiurnal arcs, adapted to

Description of an astrometer for determining the apparent situation of the stars, &c.

to the latitude of the place. In order to divide these quadrants, move the horary circle, so that 12 o'clock noon may be exactly opposite to the index B: then since the star is the equator, and its declination o, when the semidiurnal arc is VI hours, the zero of the scales of declination will be opposite VI. VI. and as the declination of a star is equal to the colatitude of the place, when its semidiurnal arc is o, or when it just comes to the south point of the horizon, without rising above it, the degree of declination at the other extremity of the quadrant, or opposite XII. XII. will be the same as the colatitude of the place, which in the present case is 39, the latitude of the place being supposed 51° North. The intermediate degrees of declination are then to be laid down from a table of semidiurnal arcs*, by placing the degree of declination opposite to the arc to which it corresponds, thus the 10th degree of south declination must stand opposite V^h 13′ in the afternoon, and VI^h 47′ in the morning, because a declination of ten degrees south gives a semidiurnal arc of V^h 13′. When the scales of declination are thus completed, the instrument is ready for shewing the rising and setting of the stars. For this purpose move the horary circle till the index B points to the time of the star's sonthing; thus opposite to the stars declination in the scale C, if the declination is south, or in the scale D if it is north, will be found the time of its rising above the horison; and the degree of declination on the scales E and F, according as it is south or north, will point out on the horary circle the time of the star setting. If the rising of the star is known from observation, bring its declination to the time of its rising on the circle of hours, and the index B will point out the time at which it passed the meridian; and its declination on the opposite scale will indicate the time when it descends below the horizon. In the same way, from the time of the star setting, we may determine the time when it rises and comes to the meridian.

The two exterior circles are added to the astrometer, for the purpose of finding the position of the stars and planets

* The most accurate table of semidiurnal arcs that I have seen, is published in the Tables de Berlin, Tom. III. p. 233.

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in the heavens. The outermost of these is divided into 360 equal parts, and the other, which is a scale of amplitudes, is so formed, that the amplitude of any of the heavenly bodies may be exactly opposite the corresponding degree of declination in the adjacent circle. The degrees of south declination, for instance, in the latitude of 51°, corresponds with an amplitude of $15\frac{1}{3}$°, consequently the fifteen degrees of amplitude must be nearly opposite to the tenth degree of declination; so that by a table of amplitudes, the other points of the scale may be easily determined. The astrometer is also furnished with a moveable index M N, which carries at its extremities two vertical sights *m n*, in a strait line with the center A. The instrument being thus completed, let it be required to find the planet Saturn, when his declination is 15° north, and the time of his southing 3^h 30′ in the morning. The times of his rising and setting will be found to be 7^h 15′, and 10^h 45′, and his amplitude 24° north. Then shift the moveable index till the side of it which points to the center is exactly above the 24th degree of the exterior circle in the north-east quadrant, and when the line A B is placed in the meridian, the two sight holes will be directed to the point of the horizon where Saturn will be seen at 7^h 15′, the time of his rising. The same being done in the north-west quadrant, the point of the horizon where the planet sets will likewise be determined. In the same way the position of the fixed stars, and the other planets, may be easily discovered.

Description of an astrometer for determining the apparent situations of the stars, &c.

If it is required to find the name of any particular star that is observed in the heavens, place the astrometer due north and south, and when the star is near the horizon, either at its rising or setting, shift the moveable index till the two sights point to the star. The side of the index will then point out, on the exterior circle, the stars amplitude. With this amplitude enter the third scale from the centre, and find the declination of the star in the second circle. Shift the moveable horary circle till the time at which the observation is made be opposite the star's declination, and the index B will point to the time at which it passes the meridian. The difference between the time of the star's

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Description of an astrometer for determining the apparent situation of the stars, &c.

southing, and 12 o'clock noon, converted into degrees of the equator, and added to the right ascension of the sun if the star comes to the meredian after the sun, but substracted from it if the star souths before the sun will give the right aseension of tbe star. With the right ascensions and declination thus found enter a table of the right ascensions and declination of the principal fixed stars, and you will discover the name of the star which corresponds with these numbers.—The meridian attitudes of the heavenly bodies may always be found by counting the number of degrees between their declination and the index B. The astrometer may be employed in the solution of various other problems; but the application of it to other purposes is left to the ingenuity of the young astronomer.

I am, Sir,

Your obedient humble Servant,

DAVID BREWSTER.

Edinburgh, April 14th, 1807.

II.

Questions and Remarks concerning the best Methods of destroying the Insects which infest Dwellings and Furniture. By a Correspondent.

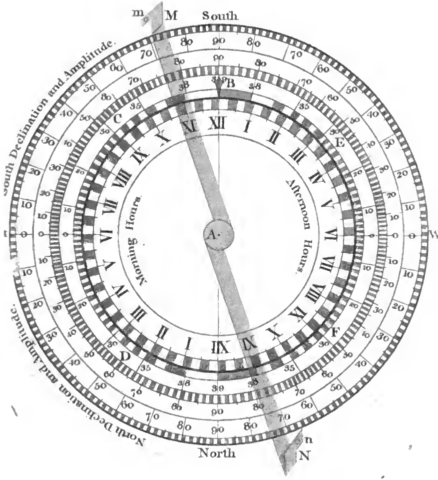
To Mr. NICHOLSON.

SIR,

Inquiry respecting the means of destroying fleas and other insects.

I LATELY read in a periodical work some enquiries by a "trifling Querist" respecting the best means of destroying or expelling those troublesome insects bugs and fleas, but particularly the latter. I was in great hope of seeing some communication that might have been useful for this purpose, but was much disappointed to read in a later number a flippant sort of answer, that we might prevent their bite by covering our bodies with tar or pitch, and that their bite might be cured by patience and letting it alone. The same means, Sir, will cure the tooth ach.—Some persons suffer very little inconvenience from the bite of poisonous insects,

Mr. Brewster's Astrometer.



insects, whilst others, from what cause I know not, suffer severely. Unfortunately for myself I am one of these. The bite of a common flea causes a very considerable degree of pain and inflammation; so great indeed as totally to disturb my rest until either the little animal is satisfied, or till I am fortunate enough to destroy it. If knowledge be valuable in proportion to utility, the means of preventing the distress occasioned by the bite of insects is not beneath the attention of philosophers. Cleanliness I know will prevent the incroachment of these vermin; but no one can guard himself from them by cleanliness of his own person, unless he can prevail on all persons with whom he has intercourse to take the same care. I wish some of your correspondents who have pleasure in the study of natural history would bestow some attention on this subject, and communicate to the public the result of their investigation. It is remarkable that different constitutions are so differently affected by the same poisons. The bite of fleas or bugs is insufferably painful to some persons, whilst others are not at all incommoded. The reason of this might be a subject of curious enquiry, But it would be an important comfort to those who suffer severely, to be acquainted with any means of protecting themselves against such distress. The common head-louse is easily destroyed or expelled from the head by combing into the hair a small quantity of white Hellibore. Whether this drug is equally deleterious to the flea, I do not know, but the experiment might merit a trial. Perhaps rinsing the blankets through an infusion or decoction of it, might render them a disagreeable lodgement to any insect. Mercury we know is in every form destructive to the insect tribe, but whether any useful application could be made of it in this case I am unable to determine. I have known the red nitrate of mercury combed into the heads of children for the purpose of destroying vermin, and I believe with complete effect. Many of the solutions of this mineral are so corrosive as might injure the texture of the clothes; but perhaps a very weak solution of the acetite of mercury, suppose a grain to a pint of water, might be used to rince the clothes through, without injuring them, or occasioning to a person sleeping in them any unpleasant effects; yet even

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even this small quantity might be so disagreeable to fleas as to expel them. Sulphur is, I believe, destructive, or at least disagreeable to insects, but I much doubt whether it could be used in any conveniont form to answer our purpose. Perhaps the sulphur-water, water containing sulphuretted hydrogen, might have some effect, but it would, in most situations, be too expensive for use. It is said that wormwood is very offensive to fleas. Probably an infusion of it might be very advantageously used to secure us against the intrusions of these troublesome little animals. As it might often be inconvenient to procure the herb fresh, it would be important to know whether the oil, or any preparation of it that could be conveniently preserved, would answer the purpose. Camphor is said to be offensive to bugs; but though I never made any experiments expressly on the subject, yet I think I am enabled by accident to contradict it. It might merit a trial, whether washing the body over with any of these articles would secure us against the bites of fleas, or bugs, or musquitoes, or gnats. With respect to the cure of these bites I can say little. I have heard military men, who have been in warm climates, speak of the custom of laying a cut lemon by the bed-side, and rubbing the part with it immediately on being bit. If any of your correspondents can communicate any useful information on this subject, I shall be one amongst many others who will feel extremely grateful at being relieved from one of the "miseries of human life."

I am, Sir,

Your obedient Servant,

A.

III.

Account of the Method and Advantage of heating Apartments and Manufactories by Steam. By Mr. NEIL SNODGRASS*.

History and account of the method of heating rooms by steam.

IN April 1798 Mr. Snodgrass was engaged by G. Mackintosh and David Dale, Esqrs. to manage a cotton mill near

* The Society of Arts gave a premium of Forty Guineas for this useful communication.

Dornoch,

Dornoch, in the county of Sutherland. He remained in Glasgow for six months after this, superintending the construction of machinery for the mill. During this period he was led to consider of a cheap method of heating the mill, as he had learnt that fuel was extremely scarce and dear in the country in which the mill was situated. It was evident that none of the methods which he had seen practised could be applied, but at an enormous expence; and his experience had pointed out to him important defects and inconveniences in them all. Having observed a mode of drying muslins by wrapping them round hollow metal cylinders, filled with steam, practised at the bleach-fields near Glasgow, it occurred to him, that by means of a proper apparatus, steam might be applied to heat a cotton mill, or any other large manufactory. It was evident that this not only would be an œconomical mode of producing heat in large works, so far as fuel was concerned, but that it would prevent the danger of fire, to which such works, when heated in the usual manner, are much exposed. He communicated his notions to a number of cotton spinners and others, from whose suggestions he expected assistance. But he met with nothing but discouragement, the project being every where treated lightly, or pronounced to be impracticable. Strongly impressed, however, with the advantages of the plan, the memorialist persevered in his resolution to make trial of it, and ordered tin pipes to be made for the purpose. These he erected in the mill in May 1799. When filled with steam they at once produced the necessary degree of heat; but the pipes, having been damaged in the carriage, proved not sufficiently strong. Indeed the memorialist was immediately sensible, that their position was unfavourable. With a view to some conveniences in point of room, they had been carried up diagonally in one end of the mill, whence the upper sides of the pipes became sooner heated than the lower, which caused an unequal expansion. The water arising from the steam condensed in the pipes in its return to the boiler, and also obstructed the steam in its ascent. In order to remedy these defects the pipes were altered, and erected in a perpendicular position, and certain tubes were connected with them, to carry off the water

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arising from condensation. The whole apparatus, as it stood after this alteration, is represented by the drawing, Fig. 1. Plate I.

This drawing presents a view of an inner gable, which is at one extremity of the preparations and spinning rooms of the mill. On the other side of this gable there is a space of 17 feet enclosed by an outer gable, and containing the water wheel, the staircase and small rooms, for the accommodation of the work. In this space the furnace and boiler are placed on the ground. The boiler cannot be shown here, as it lies behind the gable exhibited; nor is it of any consequence, as there is nothing peculiar in it. It may be of any convenient form. The feeding apparatus, &c. are in every respect the same as in the boiler of a common steam engine. A circular copper boiler, two feet diameter, by two feet deep, containing 30 gallons of water, with a large copper head, as a reservoir for the steam, was found to answer in the present instance. The steam is conveyed from the boiler through the gable, by the copper pipe B, into the tin pipe C, C. From C it passes into the centres of the perpendicular pipes E, E, E, by the small bent copper tubes D, D, D. The pipes E, E, E, are connected under the garret floor by the tubes F, F, for the more easy circulation of the steam. The middle pipe E is carried through the garret floor, and communicates with a lying pipe 36 feet in length (the end of which is seen at G,) for heating the garret. At the farther extremity of the pipe G, there is a valve falling inwards to prevent a vacuum being formed on the cooling of the apparatus; the consequence of which would be the crushing of the pipes by the pressure of the atmosphere. Similar valves, K, K, are placed near the top of the perpendicular pipes E, E; and from the middle one E, the small pipe passes through the roof, and is furnished with a valve at I, opening outwards, to suffer the air to escape while the pipes are filling with steam, or the steam itself to escape when the charge is too high.

The water condensed in the perpendicular pipes E, E, E, trickles down their sides into the three funnels, L, L, L, the necks of which may either pass through, or round, the pipe C,

C, into the copper tube M, M, which also receives the water condensed in C, C, by means of the short tubes N, N. The pipe C, C, is itself so much inclined as to cause the water to run along it to the tubes N, N, and the pipe G in the garret has an inclination of 18 inches in its length, to bring the water condensed in it back to the middle pipe E. The tube M, M, carries back the water through the gable to the boiler, which stands five feet lower than this tube. It is material to return the water to the boiler, as, being nearly at a boiling heat, a considerable expence of fuel is thereby saved.

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The large pipes are ten inches in diameter, and are made of the second kind of tinned iron plates. The dimensions of the smaller tubes are seen by their comparative size in the drawing, and perhaps they might be varied without inconvenience.

The apparatus erected as here described, has been found sufficiently strong, and has required no material repairs since the first alterations were made. The leading object in the instance under consideration being to save fuel, in order to derive as much heat as possible from a given quantity of fuel, the flue from the furnace, which heats the boiler, is conveyed into common stone pipes placed in the gable. These are erected so as to prevent any danger of fire, in the manner shown in the drawing, Fig. 2. The steam with this auxiliary communicates a heat of about 70° to the mill, the rooms of which are 50 feet long, 32½ feet wide, and 8½ feet high, except the lower story and garret; the former of which is 11, and the latter 7 feet high. The rooms warmed in this manner are much more wholesome and agreeable than those heated by the best constructed stoves, being perfectly free from vapour or contaminated air.

By various experiments it appears, that the expence of fuel is scarcely one half of what is necessary to prodnce the same degree of heat with the best constructed stoves. The memorialist was the better able to make the comparison, since he had previously had five years experience of cotton mills on what was, at that time, reckoned the most approved plan.

After

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After having ascertained these results, the memorialist, in 1800 drew a plan similar to that now presented to the Society, and sent it to Glasgow to his employers, who were very doubtful of the success of the scheme. They immediately published the discovery in the Glasgow newspapers, inviting cotton spinners, and others interested, to inspect the plan. In consequence of this public intimation of the method having been successfully practised, a number of cotton spinners turned their attention to it, and adopted it with various modifications, according to the convenience of their mills, or other notions of improvement.

The memorialist afforded to every person who desired it, all the information on the subject which he possessed. His general recommendations were to detach the condensed water, in returning it to the boiler, as much as possible from the steam; and where tin pipes, or others of similar strength, were used, to secure them carefully with safety valves.

There are obvious defects in the application of the principle, as practised in the instance described above. Of some of these the memorialist was perfectly aware at the time of the first construction of the apparatus, though it was out of his power to remedy them; and he has thought it proper to give a detail of the first successful experiment exactly as it took place.

From the pipes being all in one end of the house, the heat was unequally diffused, and a considerable time elapsed, after their being first heated, before it reached the other end of the rooms. But, as the mill had barely room enough for the spinning machinery, it was impossible to erect the pipes in any other situation, or to convey them along the rooms, so as to produce a more equal distribution of heat. This, however, can be so easily effected, when there are no obstacles, such as have been mentioned, that it is scarcely necessary to enter into any detail of the means. It may be barely mentioned that the memorialist has fitted up the apparatus in two cotton mills, which are now under his management, belonging to George Houston, Esq. and Co. of Johnstone, in a manner which completely distributes the heat. In one of these mills, consisting of six stories,

stories, a lying pipe of cast iron, five inches in diameter, is carried along the middle of the lower story, about two feet from the ceiling, with a small declivity to carry off the water. This pipe heats the story in which it is placed. Tin pipes, $7\frac{1}{2}$ inches diameter, communicating with this lying pipe, are carried up perpendicularly through all the floors to the top of the house at the distance of seven feet from each other, and form a line of heated columns in the middle of each room. The same general plan has been followed in the other mill. But there are several irregularities in the building, which require a little variation of the contrivances for diffusing the heat to every quarter. Some of the rooms having been added since the first erection of the mill, are connected with the main body of the building awkwardly. Into these the steam is carried by lying pipes, slightly inclined, and communicating with the principal apparatus. The steam may afterwards be distributed by other pipes in any way that is thought convenient. The memorialist has found no difficulty in conveying, by such means, the steam necessary to produce the degree of heat required in every variety of situation.

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In the former of the last mentioned mills, the perpendicular pipes are connected under the ceiling of the garret by a pipe $2\frac{1}{2}$ inches diameter, slightly inclined, the extremities of which pass through the walls of the house, and are provided with valves opening outwards. A connecting pipe, with similar valves, is placed under the ceiling of the third story. These are intended for the more easy circulation of the steam; but the memorialist found, from experience, that with all these aids, the filling of the perpendicular pipes with steam was attended with some difficulty. The steam, when first thrown in, passes up the perpendicular pipe, nearest to the boiler, and, being specifically lighter than air, occupies the upper part of the apparatus, compressing the air in the lower part of the rest of the pipes. The resistance of the air will thus for a long time prevent the pipes from being completely heated: but this difficulty is easily obviated by having a valve or valves opening outwards, at the lowest part of the apparatus, through which the air, when compressed by the steam, is suffered to escape.

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escape. In the mill just mentioned, the lying cast iron pipe in the first story is carried through the gables of the mill, and furnished with valves for the egress of the air. It is unnecessary to repeat, that the same valves serve for the discharge of the air in heating the apparatus, and of the steam itself, when its expansive force becomes too great. In both mills, each of the perpendicular pipes is provided with a valve, to prevent a vacuum; and in the second mill the lying pipes for carrying the steam into the detached rooms have each two valves, one opening inwards, and the other outwards.

Certificates of five other mills being heated in a similar manner, by the direction of the memorialist, are presented to the Society.

The application of the principle to buildings already constructed, it is presumed, will be sufficiently obvious from the foregoing details. In new manufactories, where the mode of heating may be made a part of the original plan, a more convenient apparatus may be introduced. This will be best explained by a description of the drawing, Fig. 2. which gives a section of a cotton mill constructed in a manner which the memorialist would adopt, were he to apply the steam apparatus to a new building, or any other that would permit such an apparatus from its regular constructions. In an old mill in this place, an apparatus is now erecting by the advice of the memorialist, conformable to this plan, which is likely to be generally adopted in new cotton mills.

The furnace for the boiler is shown at *a*. The flue of the furnace conveys the smoke into the cast iron stove pipes, 1, 2, 3, 4. These pipes are placed in a space in the gable, intirely enclosed with brick, except at the small apertures, 5, 6, 7, 8. A current of air is admitted below at 9, and thrown into the rooms by those openings, after being heated by contact with the pipes. This part of the plan is adopted with a view to prevent, as much as possible, any of the heat, produced by the fuel used, from being thrown away: It may be omitted where any danger of fire is apprehended from it, and the smoke may be carried off in any way that is considered absolutely secure. So far, however, as the memorialist

memorialist is able to judge, there seems to be little or no danger of fire from a stove of this construction. The greatest inconvenience of a common stove is, that the cockle or metal furnace is liable to crack from the intensity of the heat. By the continuity of the metal from the fire-place, an intense heat is also conducted along the pipes, which exposes them to the same accident. Here the smoke being previously conveyed through a brick flue, can never communicate to the pipes a degree of heat sufficient to crack them. In like manner the pipes, having no communication with the rooms but by the small apertures, cannot come in contact with any combustible substance; and from being surrounded with air, which is constantly changing, can impart only a very moderate degree of heat to the walls. The iron supporters of the pipes may be imbedded in some substance which is a bad conductor of heat, as furnace ashes and lime, &c. The emission of heated air into the rooms may be regulated by valves. As the pipes are not exposed to cracking, there is no risk of their throwing smoke or vapour into the rooms.

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The boiler *b*, *b*, is six feet long, three and a half broad, and three feet deep. As there is nothing peculiar in the feeding apparatus, it is omitted. The boiler may be placed in any convenient situation. Where a steam engine is used for other purposes, the steam may be taken m its boiler. The pipe *c*, *c*, conveys the steam from the boiler to the first perpendicular pipe *d*, *d*, *d*. There is an expanding joint at *e*, stuffed, to make it steam tight. The steam ascending in the first pipe *d*, *d*, *d*, enters the horizontal pipe *f*, *f*, *f*, *f*, (which is slightly inclined) expelling the air, which partly escapes by the valve *g*, and is partly forced into the other pipes. The valve *g* being considerably loaded, forces the accumulating steam down into the rest of the pipes *d*, *d*, *d*. The air in these pipes recedes before the steam, and is forced through the tubes *h*, *h*, *h*, into the pipe *m*, *m*, *m*, whence it escapes at the valve *i*, and the syphon *k*. The water, condensed in the whole of the pipes, passes also through the tubes *h*, *h*, *h*, *h*, into the pipe *m*, *m*, *m*, which has such a declivity as to discharge the water at the syphon *k*, into the hot well *n*, whence it is pumped back into the boiler.

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The whole of the pipes are of cast iron, except *m*, *m*, *m*, which is of copper. The perpendicular pipes serve as pillars for supporting the beams of the house, by means of the projecting pieces *o*, *o*, *o*, which may be raised or lowered at pleasure by the wedges *p*, *p*, *p*. The pipes are sunk in the beams about an inch, and are made fast to them by the iron straps *q*, *q*. Those in the lower story rest on the stones *s*, *s*, *s*, *s*, and are made tight at the junction with stuffing. The pipe in each story supports the one in the story above by a stuffed joint as shown at *r*. The pipes in the lower story are seven inches in diameter; those in the higher six inches; those in the other two are of intermediate diameters. The thickness of the metal is $\frac{1}{8}$ of an inch. The lower pipes are made larger than the upper, in order to expose a greater heated surface in the lower rooms, because the steam being thrown from above into all the pipes, except the first, would otherwise become incapable of imparting an equal heat as it descends.

There is no necessity for valves opening inwards in this apparatus, the pipes being strong enough to resist the pressure of the atmosphere.

The cotton mill is 60 feet long, 33 wide, and four stories high, the upper being a garret story. In the engraving five parts out of nine in the length of the building are only shown. The apparatus will heat the rooms to 85° in the coldest season. It is evident that, by increasing the size, or the number of the pipes, and the supply of steam, any degree of heat up to 212° may be easily produced. It may even be carried beyond that point by an apparatus strong enough to compress the steam; this, however, can seldom be wanted. At first it was objected to this construction, that the expansion of the pipes, when heated, might damage the building; but experience has proved, that the expansion occasioned by the heat of steam is quite insensible.*

The

* Certificates from Mr. George Mackintosh, and Messrs. Henry Monteith, Bogle and Co. of Glasgow; Mr. George Houston, Messrs. Robert Hodgart and Co. Messrs. John Fife and Co. and Mr. John M'Naught, of Johnstone; Mr. James Boyle, manager for Messrs. M'Farlam, Black and Co. at Gryse-Mill; also from Mr.

Mr. N. Snodgrass's methods of heating Rooms by Steam.

Fig. 2.

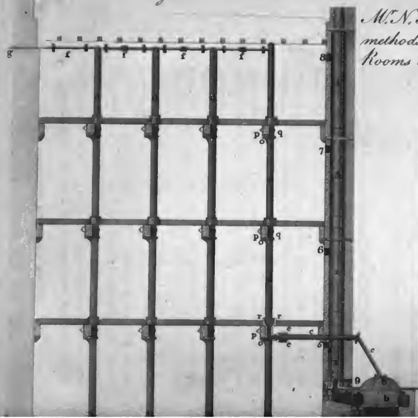
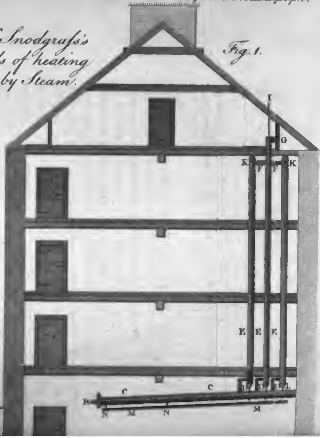


Fig. 1.



The memorialist thinks it would be improper, in addressing so intelligent a body as the Society ef Arts, &c. to expatiate on the various economical purposes to which the principle, which he has been able but imperfectly to unfold, may be applied. In abler hands it may be found susceptible of improvement, which he cannot anticipate. Experiments and observations on urine, &c.

NEIL SNODGRASS.

IV.

Experimental Inquiry into the Nature of Gouty and Gravelley Concretions. By Thomas Egan, *M. D. FRS**.

The urine of gravelly patients, when fresh rendered, nay, after standing many hours, in a temperature of sixty degrees, is relatively more acid than the healthy; sometimes as much so as the gouty; and frequently continues so, even after depositing its gravelly matter. An exception to this, however, sometimes occurs in gouty habits; their urine depositing copiously this acid substance, and yet manifesting no increased, but sometimes rather decreased acescency; for, with them, a considerable diminution of the quantity of the usually excreted super-acidulated phosphoric salt often takes place, as shall be fully explained upon another occasion.

Having premised these observations, it is now time to consider what effects acid substances are productive of, when mixed, out of the body, with this very complicated liquor. And here, to prevent repetition, I will observe, that that generally used, was rendered fresh in the morning, in the quantity of from three to four ounces, (unless otherwise specified;) being that most easily retained at one time in the bladder. The quantity of acid extremely small,

Mr. William Kerr, for the Lochwinnoch Spinning Company, confirm the utility and success of Mr. Snodgrass's method, and attribute to him the credit of first applying steam to the purpose of heating manufactories.

* Extracted from a longer Memoir in the Irish Transactions, 1805.

Experiments and observations on urine, &c.

small, for obvious reasons, and seldom increasing its acescent properties (as ascertained by the usual tests) beyond what frequently occurs, in the urine of those who use acescent drinks, or are afflicted with gout or gravel. A standard quantity was always laid by for comparison; and the temperature from sixty to seventy-five degrees, being in autumn, 1799. And to begin with the vegetable acids.

Exp. 1. To four ounces of the urine of an adult, was added one drachm of common acetous acid, which (like every other acid) caused no immediate change in it; but, in a very short time, and before it cooled down to the temperature of the atmosphere, some extremely minute shining spiculæ, observable only by a lens, were seen floating in it: these gradually increased in number and size, began to reflect the light, and, from being perfectly transparent, soon became coloured, to settle upon the usual cloud, or *nubecula*, which now began to form, adhere to the sides of the glass, and partly fall to the bottom, in the shape of small bright red crystals. In the standard, after twelve hours, nothing more observable, than the usual *nubecula;* nor was there any sign of crystalization, or separation of uric acid, even after twenty-four.

Exp. 2. To the same quantity of adult urine, were added one drachm and half of acetous acid, which caused a more copious separation and crystallization of this substance, with the foregoing appearances. None observable in the standard after twenty-four hours.

Exp. 3. To four ounces of urine of a healthy child, who never was observed to pass gravel, and of the usual degree of acidity, was added one drachm of acetous acid, which soon caused an evident and copious separation of crystallized uric acid. The crystals were, however, not quite so coloured; the urine of children not being so much impregnated with the urée, or colouring matter. No such appearance in the standard after twelve hours or more.

Exp. 4. To four ounces of adult urine, rendered very soon after a tea breakfast, and nearly in a state of *urina potus*, was added one drachm of acetous acid. After three hours, a crystallization of minute sandy particles took place. None in the standard, even after three days.

Exp.

Exp. 5. Thirty drops only, of acetous acid, were added to four ounces of the urine of a gouty patient, æt. sixty, and who sometimes felt some slight gravelly tendency. A very copious precipitation of this matter quickly took place. Some observable in the standard, also, the next day.

Experiments and observations on urine, &c.

Exp. 6. To three ounces of healthy adult urine, were added a few drops only of citric acid. A distinct crystallization, but extremely minute, took place. No appearance of any in the standard, after many hours. The experiment was repeated with one drachm of filtered citric acid, which only hastened the separation, and increased the quantity of crystalline matter.

Finding, by these experiments, and numberless others, with a detail of which it would be unnecessary to take up the time of the Academy, that the acetous and citric acids, blended with the urine, separated its uric acid in a crystallized state; I thought it might be interesting, to investigate what the effect of the tartarous acid might be: being that, which, in an uncombined, and partly combined state of acidule, as in the acidulous tartarite of potash, chiefly prevails in the wines and beverage of those countries most subject to these complaints.

Exp. 7. To four ounces of healthy adult urine, were added some drops only of pure tartarous acid. To the same quantity, one drachm of acetous acid; which brought them nearly to the same standard of acidity: a circumstance always attended to in the comparative trials with different acids. In that with the tartarous acid, the crystals were not only larger and darker coloured, but exceeded in quantity any thing before observed. In that with the acetous acid, a much smaller proportion of minute crystals took place.

Exp. 8. To four ounces of urine, were added two drachms of a filtered solution of acidulous tartarite of potash, of the temperature of 55 degrees. The usual separation and crystallization took place, in large proportion: the crystals, however, much smaller, and less coloured, than those with the uncombined tartarous acid. The two last experiments, frequently repeated, presented the same results.

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Exp. 9. The result of the above experiments having led to some doubt, as to the good effects of the carbonic acid gas, so much, at one time, recommended by Doctors Percival and Saunders, previous to its more modern alkaline combination, in our mephitic, as well as super-aerated soda waters.

Into the middle part of Nooth's apparatus, were introduced four pounds of fresh rendered healthy urine, and exposed to a stream of carbonic acid gas. After a few hours, a copious and beautiful precipitation of uric crystals took place, (notwithstanding the constant agitation, from the transmission of the gazeous bubbles,) larger than any I beforo observed, that from the tartarous acid excepted. In a standard quantity, no distinct crystallization, even after two days. A repetition of the same experiment afforded similar results.

Exp. 10. Finding the carbonic acid gas productive of similar effects, with the other acids hitherto examined; it was natural to inquire, how far its combination with the portion of alkaline matter, contained in our mephitic and soda waters, so highly surcharged with it, may prevent a separation of this uric acid.

Half an ounce only, of the common soda water of the shops, prepared by Mr. Kinsley, was added to four ounces of healthy urine. A similar quantity was impregnated with carbonic acid gas. In the former, after forty-eight hours, or more, no more than the usual *nubecula:* nor could a single crystal be discovered, even by a magnifier. In the latter, an early, copious, and beautiful crystallization. On the result of this experiment, frequently repeated, with various proportions of the mephitic alkaline water, I shall afterwards have occasion to make some remarks.

Though the mineral acids, in an uncombined state, enter not into the matter of our diet, and are no longer considered as lithontriptics, since the notion of the earthy nature of these concretions has been abandoned; yet, as they are sometimes prescribed with other indications, I thought fit to extend my researches (though in a summary way) to them also.

Exp.

Exp. 11. To sixteen ounces of urine, were added eight drops of very dilute sulphuric acid. To a similar quantity, two scruples of citric acid, to bring them to nearly the same standard of acidity. After a very short interval, in that with nitric acid, the usual appearance of transparent floating moleculæ reflecting light, and gradually becoming larger, were observed, and began to adhere to the glass: whilst in the other, after five hours, no such appearances took place. *Yet*, after forty-eight, here also a precipitation took place, of smaller crystals, and less in quantity; for, being collected on a filter, and carefully dried, they weighed only two grains; whilst the former amounted to three. And this is nearly the largest proportion I ever found the above quantity of healthy urine to contain.

Experiments and observations on urine, &c.

Exp. 12. As the nitrous acid is one of the most active solvents of this matter, out of the body, I was curious to ascertain, whether. in the very dilute state in which it must reach the kidneys and bladder, (where its action must have been facilitated, by the actual state of solution of this substance,) it would manifest its powers, in preventing its separation.

To three ounces of urine, rendered a few hours after breakfast, and, of course, scarcely acid, were added five drops of weak nitrous acid; which did not seem to add very materially to its ascescent properties.

To a similar quantity were added four scruples of acetous acid. In less than an hour, the former deposited a distinct quantity of gravelly matter, in considerable proportion. This, perhaps, we should not be surprised at, when we consider how the action of this acid, in that fluid, may be determined by superior affinity. In the latter, the separation did not take place for a considerable time after. We see, then, that the nitrous acid speedily and powerfully precipitates this acid substance.

Exp. 13. To six ounces of urine, shewing a strong acescent quality, were added only three drops of strong marine acid. A cloudiness and transparent granular precipitation took place, followed by the formation of extremely minute gravelly concretions, which, even after two days standing. did not assume so red a tinge as that with

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vegetable acids. This may, probably, depend upon some action of this acid upon the urée, or colouring matter: but, as to the smallness of the crystals, that evidently depends upon the more speedy precipitation, throwing them down before they can assume their natural size, and leaving but a shade of difference between the crystalline and pulverulent deposits.

Exp. 14. From the above, then, we are satisfied, that the vegetable and mineral acids cause a premature separation and crystallization of the lithic contents of recent healthy urine: but it may be observed, that this only takes place, under circumstances not at all applicable to the living system; viz. a much inferior temperature; and, in some instances, a contact with the atmospheric air: two powerful promoting causes of crystallization in general, but more especially of the less soluble salts. To determine, therefore, this most essential point:

To six ounces of cold but recent urine, (in a well closed phial,) were added five drops of very dilute nitrous acid, which were placed on a sand bath: temperature varying from 80 to about 100 degrees at most. The same quantity, with similar precautions, but without addition, was laid aside, in the laboratory, as a standard: temperature 56 degrees. After a very short interval indeed, and almost as soon as the urine acquired the temperature of between 80 and 90 degrees, small shining granular particles were observable with a magnifier, began gradually to settle upon a broken kind of *nubecula*, or rather *nubeculæ*, and to acquire colour and size, though carried up and down the liquor, which was in constant agitation. This experiment again twice latterly repeated, and always with the same result, (care being taken to keep the temperature, as nearly as possible, for a few hours, between 90 and 100 degrees,) afforded one of the most pleasing objects imaginable; viz. the formation of this crystalline matter, under all the disadvantages of elevated temperature, and constant agitation, from (I may almost say) their primordial moleculæ, to the accomplishment of their full size. And here, indeed, they are most beautiful, and not to be distinguished from these spontaneously deposited.

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The whole experiment strikes us strongly with a semblance of what probably passes, under similar circumstances, in nature; and reminds us of the danger attendant upon acid impregnations, more particularly at bed-time, when the urine, by many hours retention and quiet, has ample time to deposit its uric acid contents in the bladder. From it, also, we learn, that the temperature of the human body, in place of retarding or preventing (as might be expected *a priori)* these pernicious effects, rather promotes them, and that to a considerable degree.

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But whilst we endeavoured to establish this point, from practical observation as well as experiment, we seem to have entirely forgot, that the urine itself is an acid liquor; and that, therefore, if acids were so prejudicial, it is not probable, that the provident wisdom of nature would commit the discharge of this necessary excretion to a fluid, which, by prematurely separating it within the body, would completely defeat the object of her humane attention. And would she not, in the infinity of her resources, dispose of it by some less objectionable emunctory?

I would, in the first place, observe, that though healthy urine manifests the properties of an acid liquor, it is in the very smallest possible degree; so much so, that though mentioned long since, by Moraung, Coldevillars, and other surgeons, yet it was not, either chemically, or medically, acknowledged to be so, until the time of Scheele, who finally established this point, as well as the nature of the prevailing acid. And, secondly, that nothing can be more erroneous, than the opinion, which so long prevailed, that the phosphoric acid existed in it, in a naked or uncombined state. It is now well established, that it is only in that of a weak acidule, or acidulous phosphate of lime, very little short indeed of the point of saturation; and hence the weakness of its action, as an acid liquor: for were it not for litmus, and some of the more delicate of the vegetable blues, we would have been, even to this day, ignorant of this property; so very feeble indeed, that it will often not affect an infusion of red cabbage, whilst it turns with litmus, and, sometimes, but feebly, with this most delicate of all acid tests. A single drop of phosphoric

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phoric acid was added to one ounce of distilled water. Of this weak acid impregnation, one drop was sufficient to turn the infusion of litmus of as clear a red as the mineral acids do; whilst seven of urine manifested but very weak effects of acidity, and required some time to shew any. If the urine, therefore, does not exceed its natural standard of acidity, we have nothing to apprehend. And here, indeed, we must again admire the wonderful wisdom of Providence. The occasion (may I be allowed to say so, and that, too, before so competent an assembly?) required some chemical discrimination. It was necessary to carefully provide for the expulsion of the recrementitious part of the osseous fabric (which is very considerable) out of the system: but as this salt is insoluble in an aqueous vehicle, such as the urine, nothing more would be necessary, to obviate this difficulty, than a certain degree of super-saturation, or state of acidule, which would more effectually provide for its solubility, and its elimination. But by going thus far, whilst it attended to one excretion only, it would have entirely forsaken its charge of another, committed also to this fluid; and, by this degree of super-saturation, precipitate, retain in the system the uric acid, and occasion as frequent an occurrence of gravelly and calculus complaints, amongst mankind in general, as now occurs among the gouty. It, therefore, prudently formed that degree only of acidulous phosphat of lime, which, though insoluble out of the body, was sufficiently soluble, when assisted by its temperature. Nay, even for wise purposes, it has given a degree of latitude to this temperature, which, though narrow and confined indeed, is sufficient for its purposes: but where it precisely terminates I am not at present prepared to say, though so easily determined.

Let us now, for a moment, consider how far any morbid deviation, from this healthy standard, (which sometimes happens,) may throw light on this subject. The most considerable, that I am acquainted with, occurs in the instance of gouty urine, rendered towards the decline of the paroxism. A single drop of this, though in a turbid state, affects the vegetable blues, with an energy, equal, or, perhaps, superior, to that of the strongest acetous acid; and

and requires a very considerable increased proportion of lime-water to decompose it, for obvious reasons. This we find always depositing, sometimes from the bladder itself, but, generally, before it has entirely parted with its natural temperature, a very large proportion of a reddish brick-dust like sediment, (a welcome harbinger to gouty patients,) gradually declining, and keeping pace with the alleviation of symptoms, and the progressive return of the urine to its natural degree of acidity. This sediment, Scheele, Bergman, and Fourcroy, consider of the uric acid kind: and so it (but in part only) undoubtedly is; being in a smaller proportion than they were aware of. For, considering that the enormous quantity, rendered in a few days, was incompatible with the known minute proportion of this acid matter in urine, I was determined to make the following experiment. To a considerable quantity of it, desiccated and well edulcorated with distilled water, were added three ounces of a weak alkaline lixivium; which, after a few hours digestion, completely discoloured it, acquired a golden yellow colour, a sweetish taste, and, on the addition of a few drops of dilute marine acid, precipitated a copious sediment of whitish minute needle-shaped crystals, of a silky appearance. Experiments and observations on uri &c.

To this precipitate, well edulcorated, was added, by degrees, about one ounce of weak nitrous acid, which acted on it, with effervescence, and nearly took up the whole. This solution, being set to evaporate, began to redden the fingers, and other animal matters; no doubt, therefore, could subsist, as to its nature. To the remainder, which seemed very little diminished, and only deprived of colour, were added two ounces of dilute marine acid; which, after some time in digestion, nearly dissolved the whole: and, finding this acid solution precipitate with lime-water, oxalt of ammonia, and fixed alkali, it must have been phosphat of lime. This forms, then, by far the largest proportion of the gouty sediment, which is coloured by the precipitated uric acid. Such, also, is the result of Crookshank's experiments; and so we should expect to find it, as I shall endeavour to point out, on a future occasion.

Let us now consider, how far these analytical results may

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may be confirmed, in the synthetic way; having resolved, that experiment, as far as applicable, should form the basis of any opinions, offered in this essay. The phosphoric being the native acid, prevalent in urine, it was interesting to determine, whether, by the artificial super-addition of it, so as to bring this fluid to the standard of the gouty, we might not produce effects, somewhat analogous to what occur there.

Eighteen ounces of urine were divided into three equal parts. To the first were added, five drops of phosphoric acid; to the second, ten; and to the third, fifteen. In the first, the magnifier very soon discovered minute floating moleculæ, gradually assuming the crystalline form, &c. as so often before described. In the second, the same appearances, but more immediately and copiously produced. But in the third, so considerable, as to excite my astonishment. For here, besides the same extremely minute crystals, which adhered to the entire sides of the phial, the bottom appeared covered with a mixture of crystalline, and red pulverulent matter: the latter in a great proportion, and, probably, prevented from crystallization, by its hasty deposition. Here, then, that encreased proportion of calcareous phosphat and animal gelatinous matters, (which always takes place in gout, and could not be expected here,) would seem only wanting, to form a sort of synthetic approximation to the gouty sediment.

The unusual proportion of deposited uric acid, in this experiment, created some suspicion, that the phosphoric acid might, by a combination with some of the principles of this very compound fluid, give rise to some artificial formation of it on this occasion.

To the filtered liquor, therefore, of Number 3, were again superadded five drops, which, in twenty-four hours, caused a farther separation of a very few crystals only.— It was filtered a third time, and eight drops more added; but without the smallest appearance of a single crystal, after four days. The additional acid, then, only more effectually and speedily determined the separation of the quantity, naturally contained in urine: its more divided pulverulent appearance adding considerably to its volume.

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It now only remained, to demonstrate the identity of these various precipitates, with the naturally deposited matter of gravel. For, though it could not be well mistaken, for any other saline composition in urine; yet, as external characters are, even in the hands of a Romé de Lisle, or an Abbe Haüy, fallacious, the following, and concluding one, on the subject of acids, was instituted. Experiments and observations on urine, &c.

Exp. 15. To two drachms of this artificial gravelly matter, was gradually added one ounce of nitrous acid; which acted on it, with effervescence, and dissolved the whole, with the exception of some small, floating, floculent, animal particles, so well described by Bergman.

The evaporated solution reddened the skin, and, after some time, deposited crystals of oxalic acid; as happens in all concentrated nitrous solutions of calculi, of the uric acid kind. To another small quantity, was added some pure alkaline lixivium; which very soon took it up, became coloured, sweetish, and deposited the usual silky crystalline sediment, upon the addition of acetous acid. No doubt, therefore, could remain, as to its identity, with that naturally deposited

And here, though irrelevant to my present object, and merely with a view to excite the attention of the faculty, may I be permitted to ask, how it happens, that, in the very worst kinds of typhus fever, there is very little diminution of the secretion, or excretion of the acidulous phosphat of lime? as appears by the acidity of the urine, lime-water, and the *quantum* of precipitate, afforded by the oxalic acid: whilst a very considerable one of the uric acid takes place, and continues so, until nearly the termination of the disease, when it begins gradually again to manifest itself; first, by the usual tests only; but presently, upon the crisis taking place, in such quantity, as to become insoluble; and, therefore, quickly precipitates, (with some additional mixture of calcareous phosphat, and animal mucilaginous matter,) under the form of our critical sediment or deposit? Or, are we not here, again, to admire the wise economy of the Author of nature, which, by keeping up the considerable and necessary bony excretion of the system, prevents the dangerous accumulation of it, which must ensue,

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sue, from its retention, during the long protracted period of many fevers? I might here offer some conjectures, in explanation, but will reserve them for another place.

Having already trespassed so much upon the indulgence of the academy, I shall here content myself with briefly stating, that, from the above experiments and observations, we may presume to say, acids of every kind are prejudicial, and give rise to the formation of gravelly and calculus affections, by causing a separation, and crystallization of the lithic acid contents of urine, within the body: not pretending, however, to deny the existence of other causes, inherent in the system itself, occasionally productive of similar effects, as has been already observed.

V.

An Essay, Physiological and Experimental, on the Effects of Opium on the living System. By WILLIAM ALEXANDER, *M. D**.

Effects of opium on the living system.

IN this paper, which I have the hononr to submit to the consideration of the Society, I propose to enter into an examination of two questions, *viz.* 1st. *Can the effects exerted upon the living system, by the operation of Opium, be accounted for, without the agency of the nervous system?* 2d. *What is the nature of that operation, whether sedative or stimulant?*. This subject I do not select either because I have some new doctrines to establish, or because the generally received opinions concerning the operation of opium, require additional support, but rather because in the discharge of a duty for a long season neglected, I am obliged to have recourse to those means, which the present opportunity allows me.

In this disquisition, it may be considered, that I enter upon the investigation of a subject, which has already been rendered barren by the diligence of preceding enquirers, and that consequently nothing of novelty can be expected.

* Abstracted and abridged from the Manchester memoirs. New series. Vol. I.

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It is not under this expectation that I take up the pen, for how much soever may have been effected, something yet remains to be done by the diligent and patient enquirer, and, though nothing could be gained beyond a confirmation of established opinions, yet if this be done through the means of accurate and repeated experiments, something is added to the stock of information, and it must be considered at the least as possessing a relative value. Effects of opium on the living system.

These experiments are not however destitute of some novelty in the arrangement, and they will be found to exhibit, in a clear analytical succession, the effects produced by opium upon the different parts of the animal machine. But it is not clear by any means, that the physiologists of this day are agreed upon many points, which will be brought forwards in this essay, and more is required to be done, before the subject can be considered as exhausted.

The humoral pathology, which had for a long space of time occupied the schools of medicine, had no sooner been called in question than a variety of opponents arose in every quarter against it; the new opinions being clothed in professional authority and enforced by the learning and genius of several private teachers, the tide of opinion flowed in a contrary direction, and it became the fashion to account for all, or most of the deviations from a state of health in the animal body, from some primary alteration in the condition of the solids. In many points the advocates for the new doctrines were notwithstanding at issue with each other, and the memorable contest betwixt Haller and Whytt, respecting the origin and nature of irritability, opened the physiologist new sources of enquiry and laid new foundations for future improvement. The agency of the nervous system, which was still necessary for the explanation of most of the phænomena upon the theory of diseased solids, began at length to be exploded by the advocates of another yet more refined and simplified, which the creative genius of John Brown ushered into the schools of physic.

This doctrine rejecting the explanation of diseases upon partial and confined theories, attempted to refer all the various changes in the human body to one general law. He

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He maintained the existence of a principle in the animated body, which he denominated excitability; That this principle was characteristic of life; that action was the consequence of the operation of certain powers upon this principle, health the consequence of the due and proper operation of these powers, and disease the effect of the abundant or deficient action of these powers.

In this state of things the very accurate and most philosophical thesis of Dr. Goodwin, upon the cause of death from suspension and submersion, made its appearance, in which he plainly proved the existence of a primary change in the condition of the blood; that this condition was sufficient, and indeed necessary to occasion death. About this period also the experiments of the celebrated Italian philosopher, Fontana, attracted considerable attention and became the subject of much discussion. He contended from numerous experiments, that opium was a power, which exerted a direct influence upon the blood, or that the blood was a necessary agent to communicate its operation to the living and irritable fibre, and without the circulation of which, the usual effects of opium could not take place. His experiments, which excluded the agency of the nerves altogether in producing the general effects, resulting from the exhibition of opium, afforded considerable support to those who maintained some new doctrines of irritability *.
This

* These physiologists rejecting the nosology and practice of Dr. Brown as incompatible with his fundamental principles, but adopting these, and using the borrowed term of irritability instead of excitability, attempted to establish a new hypothesis, by explaining all the changes, which the body underwent in a state of health and disease, upon an alteration in this principle. The experiments of Fontana, which went to deny the influence of the nerves, coinciding with this new hypothesis were eagerly embraced by them.

The manner in which these physiologists explained the consumption of irritability upon the application of a stimulus, without the agency of the nerves was somewhat curious. They supposed the principle of irritability was like the matter of heat, diffusible over every part of a body endowed with it, that when any portion of it was destroyed by the action of a power applied to any part,
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This property was considered not only as not being derived from the nervous system, but capable of being encreased, diminished, or exhausted by the application of external powers, which had no effect upon the nervous system, and that it was, to use the words of Dr. John Brown, as applied to his principle of excitability, "Una toto corpore et indivisa proprietas." Effects of opium on the living system.

To ascertain how far some of these opinions were consistent with the laws of the animal œconomy, I instituted a set of experiments, which formed the subject of an inaugural dissertation, published in the year 1790. It appeared to me in consequence of that investigrtion, that several of the above-mentioned opinions, *viz.* That opium did not act upon the nervous system; that it acted upon the blood; that its effects conld be extended by means of the one and indivisible property of irritability, had been founded upon reasons which were very unsatisfactory.

This publication being calculated principally for the meridian of Edinburgh, was confined to that place, and the question, taking in a general point of view, was left undetermined.

Since that time I find, from the perusal of a work, called "Medical Extracts" written by a gentleman of some ability, but of more *imagination* than judgment; that the opinions of Fontana are not only sanctioned by respectable authority, but are considered as generally known, understood and acted upon. I have, therefore, thought it necessary to collect into a short point of view the facts, related by Fontana, and the general conclusions he drew from them, and to compare them with the principal facts, established by the investigation above alluded to.

"I* destined, says Fontana, 300 frogs for these experiments, and by means of pincers and scissars, I laid bare

the expenditure, thus occasioued, was supplied by the influx of a new quantity from the general stock in the system; thus the continued action of a stimulant power, keeping up a continued expenditure, there would be a succession of new influxes until the whole irritability of the body was consumed by the repeated wants of that part to which the destructive agent was applied.

* Medical Extracts 630. Vol. 3.

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the crural nerves in such a manner, that they were entirely free of every other part, and obtained about eight or ten lines of nerve totally clear and in some very large frogs even more. I then let fall the nerves of each thigh into a small hollow glass, which received them in such a way that I can fill each glass with a fluid of any kind without its touching the adjacent muscles. I usually have been able to put into these glasses, such a proportion of whatever I wish to try on the nerves, as to cover the greater part of them with it, without its being possible for any of the liquor to find its way to the thighs and mix with the blood. In this way I can make a comparison betwixt the nerves, that are envenomed and those that are not; compute the time they continue to contract the muscles, and judge of the vivacity of the motions."

"At the end of the first ten minutes I stimulated the medicated nerves, *i. e.* those, to which the solution of opium was applied, and those which were not medicated, and found that the two extremities, the right as well as left, contracted with the same force and vivacity."

"At the end of twenty minutes, I tried the stimulation, and could perceive no sensible difference betwixt the motions of the two feet, which were almost as lively as those in the first experiment."

"At the end of thirty minutes, the motions of the two feet were feebler, but alike in both."

"At the end of forty minutes, the feet scarcely contracted, but their distinct muscles were clearly seen to contract, when the crural nerves were stimulated, and the motions of these muscles were equally lively in each foot."

"At the end of fifty minutes, the motions were very small 'from compression of the nerves,' but alike in both sides."

"At the end of eighty minutes, there was no longer any motion to be observed in several of the frogs, in whatever way I stimulated either the crural nerves that were medicated, or those which were not so."

"I can conceive," adds Fontana, "nothing more decisive and more certain than from this series of experiments, that the action of opium is not directly on the nerves."

2dly.

2dly. Again,* Fontana immersed the hearts of various animals immediately taken from the thorax, into a strong aqueous solution of opium, infusion of bark and simple water, of equal temperatures, and found that these organs were deprived of irritability, and that they ceased to contract, or to be capable of being excited to contract, equally soon on immersion into water as into a solution of opium or infusion of bark.

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3dly. He next injected an aqueous solution of opium into the jugular vein of several rabbits, and found that it produced death instantaneously; from this he concludes, as the heart is not furnished with nerves, and having proved that the solution of opium does not exhaust the irritability of the heart, that it must occasion death only by producing an alteration in the condition of the fluids.

The experiments with the 300 frogs, as related by Fontana, I repeated, though upon a smaller scale, yet sufficient to ascertain the truth of it. I followed the method described by Fontana, and I found the fact to be correctly as he relates it; the divided extremity of the crural nerves, bared for the space of half an inch, and immersed in solutions of opium of various degrees of strength, was not more affected than if the same nerves had been immersed in water, and the irritability of the muscles, to which they were distributed, was not in the least degree more altered.

Although I admit the accuracy of these experiments, I am inclined to call in question the sufficiency of them for the purpose they were designed. There is a considerable difference betwixt the sentient and the divided extremity of a nerve. This operation, even if the structure of the divided part was capable of receiving and communicating impressions, must in a great measure have had the effect of destroying its sensibility, and though the solution was not only applied to the divided extremity, but also enveloped the surface of the nerve for a considerable distance, this surface must also have lost in consequence of being separated from the muscles by "scissars and pincers" so much of the usual quantum of sensibility as to be unequal to transmit any effect produced upon it.

* Fontana on Poisons, Vol. II. p. 352–364. French edition.

Respecting

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Respecting the second series of experiments, they are so contrary to all that repeated experience has taught, so contrary to the observations of Haller, Whytt and Munro, who, notwithstanding the difference of opinion they held, on some points connected with the operation of opium, unequivocally agreed on this head, (viz. that it destroyed the irritability) that I cannot but conjecture, some unobserved circumstances must have diverted the usual accuracy of Fontana from its natural bias.

The conclusion drawn from the third series of experiments rests partly upon the accuracy of the second, and partly upon the supposition that the heart has not any nerves, which is concluded because the knife of the anatomist has not discovered them; but except this opinion is maintained upon some other ground, it can be considered only as a *petitio principii;* the want of detection proves nothing either way, as it is nothing more than an argument of non-existence drawn from invisibility. Further, the experiment proves too much; the animal died instantaneously, on the injection of the solution into the jugular vein; the circulation must of course be interrupted; by what means was this sudden, this momentary effect communicated to the distant parts of the animal?

I have thus stated the proofs and arguments founded upon them, adduced by Fontana, as accurately and at as much length, as the limits of this paper will allow; let us now see how the case stands when reduced to the test of experiment.

Does Opium act upon the Irritability of the Muscular Fibre?

Exp. 1.* The heart of a frog was immersed into half an ounce of an aqueous solution of opium, in the proportion of half a dram and six grains of opium to one ounce of water, of the temperature of 44°, whilst contracting 25 times in a minute. Two minutes after immersion, it contracted only 15 times in a minute: after 8 minutes the contractions had ceased, and could not be excited again by any mechanical stimulus.

Exp. 2.† The heart of a moderate sized rabbit whilst

* Vid. p. 5, Inaugural. Dissertat. Exp. 1.

† Vid. Inaug. Dissert. p. 7. Exp. 4.

contracting

contracting 23 times in a minute, was immersed into an ounce of the above solution, of the temperature of the room; four minutes after immersion, it exhibited eighteen contractions in a minute: ten minutes after immersion, six or eight contractions, and after twelve minutes had entirely ceased, and could not be excited anew to contract. Effects of opium on the living system.

Exp. 3.* Another heart immersed in an ounce of a stronger solution, only exhibited three or four strong contractions, on the period of immersion, and afterwards was irritated in vain.

Exp. 4.† The heart of another rabbit was placed in a wine glass and three drams of the strong solution poured on, whilst contracting 50 times in a minute: after three minutes spontaneous contractions had ceased, but irritated with a needle a few contractions were excited: after the lapse of five minutes no contractions could be excited.

In order to examine how far the opium contained in the solution contributed to produce the above rapid exhaustion of irritability,

Exp. 5.‡ The heart of a frog, contracting 24 times in a minute, was placed in half an ounce of water, temperature 44°. It continued to contract in the water for 16 minutes, and when removed, contractions could be excited in the organ, by mechanically stimulating it, for the space of *six minutes longer*.

Exp. 6.§ The heart of a rabbit contracting 50 times in a minute, was placed in water of the temperature of the room. It contracted spontaneously for 20 minutes, and when removed continued irritable for the space of 10 minutes longer.

Can the Effect of Opium be communicated to distant Parts independent of the Assistance of the Circulation?

Exp. 7.|| The sternum of a frog was carefully elevated and the heart removed, forty drops of the strong solution were injected into the stomach. In 15 minutes the animal

* Vid. p. 10. Inaugural Dissert. Exp. 7.

† Vid. p. 10 & 11. Exp. 8.

‡ Vid. p. 6. Exp. 3. Inaug. Dissert.

§ Vid. p. 8. Exp. 6. || Vid. p. 28. Exp. 20.

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was stupified and paralytic, in 20 minutes convulsed: after 40 minutes, voluntary motion had ceased: after an hour and ten minutes it was dead and the irritability in all the muscles was destroyed.

Exp. 8.* Thirty drops were injected into another frog, after the removal of the heart; it lived an hour and 15 minutes, and after death the irritability was exhausted.

Exp. 9.† Twenty drops were injected into a third: It lived an hour and twelve minutes, and the state of irritability was the same as in the preceding.

Does the Quantity of Opium sufficient to occasion Death, effect this by inducing a Change in the Condition of the Blood?

Exp. 10.‡ By some former experiments, No. 17 and 35, § it had been found that 33 drops of the solution of opium injected into the jugular vein of a rabbit would occasion death in the course of a few minutes, and exhaust the irritability of the muscular fibre. Another rabbit was selected, and 33 drops injected into the crural vein; no other effect resulted from this but some degree of stupefaction. Twenty-six minutes afterwards 33 more drops were injected into the crural vein of the other limb.

The animal in a short time became more languid, but was not convulsed; its pulse was rendered more slow and feeble, at the period of 36 minutes from the injection into the first crural vein.

Seven hours from the first injection, the animal was convalescent, and the day following it fed as usual.

The occasion did not offer to make a computation of the quantity of opium which would be necessary to kill a rabbit when introduced by a crural vein, but the omission of this does not detract from the force of the evidence which the above experiment supplies, that the cause of the death of the animal, when the solution is introduced by the jugular vein, must arise from some other state, than a change in the condition of the blood, and that the effect of opium.

* Vid. Inaug. Dissert. p. 29. Exp. 21. † Vid. p. 30. Exp. 22
‡ Vid. p. 74. Exp. 46. § Vid. p. 20 & 55.

must

must have been extended over the entire system, by other means than the circulation; for, what reason can be given why the mass of fluids should not be altered, when the solution was introduced by the crural as well as by the jugular vein; but upon the other theory, the solution of this difficulty is easy, and accords with the whole series of experiments. * Effects of opium on the living system.

The life of a rabbit cannot be sustained a minute without the action of the heart; when the solution of opium is injected into the jugular vein, it is applied to the inward surface of the heart, mixed with a very small quantity of blood, and can then exert effects upon that organ as instantaneously as if the heart was immersed in it, as in experiment 2d †, the action of opium being thus directed against the irritable fibre, the exhaustion of that part would immediately succeed, of course the animal must die; but when the same fluid is injected into the crural vein, it does not reach the heart until it has been mixed and diluted with a very considerable portion of blood, so that no quantity of blood which the heart could contain during one period of dilatation, would be impregnated with any great quantity of the solution of opium. The consequences therefore, which followed from the injection of opium into the jugular vein, supposing that it acted immediately on the heart, could not in this instance he expected to take place.

Does Opium act upon the Nervous System?

Exp. 11. ‡ A triangular piece of bone was taken from the craninm of a frog, and the dura and pia mater removed; eight drops of the strong solution were injected upon the brain; a few drops were lost. In one minute the animal was convulsed, in three minutes it was dead. On examination the irritability in every part of the voluntary muscles was destroyed, neither compression of the nerves nor mechanical irritation could excite any contractions in them. The heart had not lost its motion.

* Vide Inaug. Dissert. p. 119. Note C.
† Vide Inaug. Dissert. p. 17. Exp. 14.
‡ Vide Inaug. Dissert. p. 49. Exp. 29.

Effect of opium on the living system.

Exp. 12 *. In another experiment of the same kind, the animal was immediately and generally convulsed, and was dead in one minute. When the heart was exposed it was found contracting 42 times in a minute. The irritability of this organ was not lost until three hours after.

Exp. 13 †. A portion of the cranium of a rabbit was elevated in like manner, and forty drops injected on the surface of the brain. At first the animal appeared lethargic and tottered. After ten minutes it was violently convulsed, and in the space of one minute and an half more, was dead. When the thorax was opened the heart was found contracting with considerable force.

The irritability was exhausted in all the muscles subservient to voluntary motion; they were repeatedly irritated, but in vain.

In these experiments, it is clear, that opium has a very powerful and instantaneous action upon the brain, that it is diffused over the whole nervous system, evinced both by the general convulsions preceding death, and the total consumption of irritability in the voluntary muscles, and which was equally as complete as if the opium had been applied immediately to the parts themselves.

It was next examined, if when opium is introduced into some other organ, its effects are extended by the nervous system to distant parts.

Exp. 14 ‡. All the parts as near as possible to the pelvis of a frog, on both sides, were divided, leaving the ischiatic nerves uninjured. These were afterwards secluded from the air, by the divided edges of the skin being drawn together by slender threads. Three frogs were experimented upon.

Twenty drops of solution were injected into the stomach of one frog. The animal lived four hours after.

On examination after death, the irritability was destroyed in all the voluntary muscles.

Into another frog thirty drops were injected: After sixteen minutes the animal was convulsed; the extremities below the knee which had no communication with the

* Vide p. 50. Exp. 31. † Vide p. 51. Exp. 33.
‡ Vide Inaug. Dissert. p. 59. Exp. 38—39.

superior

superior part of the body, except by the undivided ischiatic nerves, were likewise affected by convulsions, and in two hours and ten minutes the animal was dead. Effects of opium on the living system.

The ligatures which united the divided edges of the skin of the thighs were then separated, and the ischiatic nerves exposed; they were compressed; the compression of the nerve on one side, produced a slight contraction in one of the muscles in the lower part of the limb, but when repeated, no contraction followed; the compression of the nerve of the other limb, occasioned no contractions. The irritability in all the other muscles was exhausted.

Into a third frog, prepared in like manner, forty drops were injected: after fifteen minutes the animal was convulsed; after the space of two hours it was dead. Compression of the nerves did not excite the least motion in any of the muscles beneath, and when the skin was removed, the application of salt was equally as ineffectual, not the slightest degree of contraction was rendered visible.

From the event of these last related experiments we are instructed, that the effect of opium is extended to the most distant parts of the body, although the only communication which remains between the extreme parts and the body itself, is by the continuity of nerves, and these palpably not in a state best adapted to convey impressions.

It yet remained to be examined if by any other communication the effect of opium could be extended to distant parts, if the supposed integrity and indivisibility of the irritable principle was capable of doing it.

Exp. 15 *. The spine of a frog was divided above that part from whence the nerves issue which supply the inferior extremities; care was taken not to wound any other part.

After this operation, the muscles of the inferior extremities retained their irritability, and though the animal had lost the power of voluntary motion in them, it had strength to drag them after its body.

Into the stomach of a frog thus prepared, forty drops of the solution were injected; seventeen minutes after, all the parts of the animal above the point of the division of

* Vide Inaug. Dissert. p. 62. Exp. 42.

the

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the spine, were violently convulsed, and in one hour and forty minutes the animal was dead.

The upper part of the body was then separated from the lower, at the part where the spine had been divided, and the following was the state of irritability in the different parts.

To the muscles of the breast and those of the superior extremities, salt was applied, but without exciting the least contraction or motion.

The iliac nerves below the point of the division of the spine were compressed; vivid and frequently repeated contractions were excited in all the muscles of the legs and thighs.

This experiment was repeated several times and invariably presented the same result. All the muscles of the body above the point of the division of the spine, lost the irritability; on the contrary, below the point of division, the irritability of the muscle remained unimpaired after the death of the animal, as was rendered evident both by the compression of the nerves and the application of salt.

To examine these circumstances a little more minutely, another experiment was made.

Exp. 16*. The ischiatic nerves were divided on both sides in a frog, near their exit from the pelvis; this operation does not render the limb entirely paralytic. The animal still possesses a voluntary power over the muscles of the thigh, in a considerable degree: The upper point of the leg is rendered nearly paralytic, the lower point of the leg and feet are rendered entirely paralytic.

Into the stomach of a frog, thus prepared; thirty drops of the solution were injected; after twenty-one minutes the animal was convulsed; the convulsions extended to the thighs; the legs and feet were not in the smallest degree affected by convulsions. In one hour and eighteen minutes, the animal was dead.

The application of salt to the inferior extremities, the lower joints of the legs and feet, produced rapid and frequent contractions; the muscles of the thighs at first did

* Vide Inaug. Dissert. p. 66. Exp. 44.

not

not contract, but after salt had been applied some time, feeble contractions were excited. The salt applied to the muscles of the superior extremities and to those of the breast and back, was incapable of exciting the smallest degree of contraction.

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In this manner I submitted the experiments and opinions of the Abbé Fontana, to an accurate investigation, and I did not draw conclusions different from his without the conviction that the experiments which I have related were carefully made and many times repeated, and in the presence of those whose bias led them to favour the opinions of the Italian physiologist. I shall therefore conclude this part of the paper, with a general enumeration of the facts which have been ascertained.

The first series of experiments proves that opium applied to the muscular fibre (the heart) exhausts or consumes, the irritability of that organ. *Vide* Exp. 1st. 2d. 3d. and 4th.

The second series of the above quoted experiments proves that the effect of opium is transmitted to distant parts of the animal body, when the agency of the circulation is both withheld and destroyed, and in as rapid a manner as when the circulation of the blood is entire and vigorous. *Vide* Exp. 7th. 8th. and 9th.

The third proves to a certain extent, that opium either does not exert any immediate action upon the blood, or that this fluid is an insufficient medium to convey it to distant parts of the system. *Vide* Exp. 10th.

The fourth series proves that the effect of opium is directly exerted upon the nervous system. Exp. 11th, 12th, 13th. That in proportion to the unity and integrity of this system, the effects of opium are extended to distant parts. Exp. 8th. and 9th. That where this integrity is only partial, the effects are only partial. *Vide* Exp. 16th. That where the integrity is interrupted, the effect of opium is interrupted. Exp. 15th. And finally, that the *una et indivisa proprietas* of irritability is inadequate in any degree to extend or communicate the effects or operation of the above-mentioned power *.

VI. On

* In the dissertation which has been so often quoted, the above experiments will be found supported by many others, the tendency

 of

VI.

On the Methods of improving Poor Soils, where Manure cannot be had. By John Alderson, *M. D.**

Observations and enquiries respecting the improvement of poor soils.

By soil is generally understood so much of the earth's surface as has been acted upon by the sun and air, and impregnated from time to time with the result of vegetable and animal decomposition; but, as some plants will grow where no such impregnation has taken place, we shall consider this as *mould*, and define soil, a compound of certain proportions of the simple earths, of which Naturalists reckon six or seven; and as three of these generally prevail, it will be quite sufficient for my purpose to note, that these three are, chalk, flint, and clay. With chalk and clay every person is acquainted; and the common mode in which flint affects the agriculturist, is in the form of sand.

All writers and experimentalists have agreed, that none of these earths will, separately, answer the purposes of

of all which verge to the same point. In that, the general criterion which was established to denote the influence of opium, was founded on the observation of convulsions preceding death, and the loss of irritability in the muscular fibre after death. The quantity of this remaining was denoted by the frequency and strength of the contractions upon the application of common salt. It was, after many trials with other substances, found to be the most certain and effectual test. The manner in which salt produces this effect is no less beautiful than singular. It does not so much appear to excite a muscle to contact, because a certain portion of irritability is present, as it appears to bestow irritability, if this principle is not too much extinguished and the vitality gone. A muscle which is incapable of contracting on the application of a mechanical stimulus, and is relaxed and pale, will, on the application of salt, exhibit very frequent and strong contractions, assume gradually a beautiful florid colour, and will then become obedient to other stimuli, to which before it was insensible. Thus it will be found to be a better restorer of irritability to the muscular fibre, than muriatic acid, related by Humbolt.

* Extract from a Memoir read in the Holderness Society, and published by Harding, St. James's Street.

agriculture;

agriculture; but that, when properly mixed, they certainly do support the roots and add to the growth of plants; and according to the best information on the subject, if taken generally, the soil, when divided into eight parts, ought to consist of the following proportions;—three parts of clay, three of chalk, and two of flint, in the form of sand; this last admitting of great variation with respect to its fineness or coarseness, according to the nature of the climate*.

Observations and enquiries respecting the improvement of poor soils.

Many plausible reasons have been assigned why this admixture of the earths is necessary for the purpose of forming a good soil. First, a soil consisting entirely of clay, would not part with its water sufficiently; chalk would part with it too fast, and flint would not retain it at all. Secondly, there are many of the plants we wish to cultivate, whose tender fibres are not able to penetrate clay; others that will not be sufficiently at rest from the loose and changeable nature of sand; and others that cannot act upon chalk †.

If, then, the fertility of soil depend upon the due admixture of the various earths, we may safely infer, that its sterility, or poverty, proceeds from the want of that combination. If land be barren when consisting of only one species of earth, its poverty will be in proportion the superabundance that the soil possesses of that species, let it be flint, clay, or chalk.

Experimentalists having then agreed, that a due mixture of the earths is necessary to form a fertile soil, and that barrenness proceeds from a want of the proper proportion, we see the necessity of being precise in our description of the soil we call barren.

* These proportions will differ according to the quantity of rain that commonly falls in any given place. I need not here enter upon the reasons why more rain does fall in one place than another; the fact is indubitable, and I recommend the placing rain-gages in different parts of the country, in order that, by comparing the result with the experiments now carrying on in other countries, we may be enabled to say what it the best proportion for this district.

† See Kirwan, on Manures.

Observations and enquiries respecting the improvement of poor soils.

If I am asked how to improve a certain field, I should immediately wish to ascertain what is the nature of its soil; in which kind of earth it is deficient; and in what it superabounds.

If it be all clay, its proportion of chalk and sand must be added; and, where these cannot be had, substitutes may perhaps be found: stiff clay soil is made more open, in some countries, by burning portions of it in heaps, and then ploughing the hardened earth into the land.

If the soil be sand, a frequent source of barrenness in different parts of Suffolk (where I have seen whole acres of barley blown away,) then clay becomes useful, and marl the best possible ingredient *.

An ingenious man, having obtained a grant of some waste sandy land, which, till then, had been wholly unoccupied, was allowed to enclose as much as he could cultivate. He found near the foot of a hill a stratum of clay, with which he covered, the first year, an acre of sand, and then sowed it with grass seeds; this succeeding, he followed up his plan year after year, till he formed a complete surface of grass on many acres,—which, ploughed up last year, produced him nine quarters of oats *per* acre. Thus land, which, but seven years ago, would not have maintained a single sheep, became fertile, and of considerable value †.

These premises being granted, and the facts established on the authority of many and repeated experiments; let us see, if any theory can be formed to account for the circumstance, why a mixture of the earths should be necessary for the purposes of agriculture.

The changes which take place in combustion, and those changes which constitute or exhibit animal and vegetable life, have often been compared: Food which supports fire, (as oxygen) is well known to contribute to the support of

* Common marl contains from 66 to 80 parts of pure chalk; the remainder is in various proportions pure earth of alum and silex. *Kirwan.*

† This system, on a much larger scale, has been pursued by one of the most intelligent farmers in Suffolk, Mr. Rodwell, for which he obtained a medal from the Bath Agricultural Society.

life;

ife; and their products are in many instances the same (as carbon.) Now, in order to illustrate the present subject, I would carry the comparison further than it has hitherto been done, and I would draw an inference by analogy from the process of fusion, and shew how requisite it is to make a due mixture of earths for the support of vegetable life, from the necessity there is of mixing these very earths in certain proportions, in order to render them capable of being acted upon, so as to be chemically combined, by means of fire.

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If I put pure clay, chalk, or flint into a crucible, and place it in the hottest part of a furnace, no alteration or change takes place; it will indeed lose the water or air that was attached to it, but the earth will remain the same, for it is perfectly irreducible: if, however, I mix them in certain proportions, and then apply the same degree of heat, they will liquify, continue in a fluid state (so long as the fire is kept up) and their particles, intimately combined, will form a mixt mass with properties distinct from each in its simple state.

Now the operations of vegetable life resembling, as we said before, the chemical processes of combustion, may not a due mixture of these earths, when presented to the mouths or radicles of plants, render them equally capable of being absorbed, and converted by the action of the living principle into food, as they are of being fused or rendered liquid by fire? And thus am I not justified by the analogy, to draw this conclusion, that, by such an union plants derive their nourishment from the earths?—for, if the contact of these different particles of earth be alone necessary to enable the fire to produce the wonderful difference between the state of a fluid and a solid, it is difficult to be conceived, that the principle of life, so analogous to fire, should be able to exhibit similar effects, in similar circumstances; and, taking advantage of the state of the earths, when thus duly proportioned and mixed, be able to absorb and convert them into nourishment? We see also from this theory, the philosophy of ploughing, harrowing, hoeing and rolling, operations indispensably necessary to a good

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good system of husbandry. Whenever plants have drawn from the soil, in the neighbourhood in which they are placed, all the materials that happen to be duly mixed, they are no longer capable of thriving, until, by a new operation, more particles are brought into contact with them. This has been sufficiently proved by persons who are in the practice of horse-hoeing, and is in effect the very object of those repeated ploughings which are performed with the view of preparing the ground for the reception of fresh seed. By this theory we see why marl becomes so admirable an addition to some soils, as to be even called a manure. Marl is formed by the deposition of clay *, and chalk from water, which, during floods and rains, has held these earths suspended, and which component parts are so intimately combined, as to be capable of being acted upon by plants. Marl I apprehend will be found in this neighbonrhood at some future time, when repeated boriugs shall have given us the exact state of the different strata of this district †.

If I shall have the good fortune to establish this theory, we shall not have occasion to seek for the reason why chalk renders clay productive, by supposing that the latter contains an acid ‡, which the chalk absorbs, for that would be begging the question, as no such acid has been proved to exist, nor shall we have any difficulty in accounting for the different opinions of authors upon the value of lime, chalk, &c. as improvers of the soil; for, when the lime has exerted all its powers as a manure, (that is, such of it as has suffered decomposition through the medium of water, in which, till it recovers its air, it is soluble) the remainder being mere chalk, mixes with the soil, and, as it may happen, will be useful or not according to the nature of the ground it is laid upon. Lime may answer to the farmer as a stimulus, but it can only improve the soil to the land-owner, when it is laid upon clay or

* All clay contains a portion of sand.

† Vide Kirwan's Mineralogy.

‡ See Home, Mills, and others.

sandy

sandy soil: and in this view, chalk, in an equal state of fineness, is as valuable as lime*. Observations and enquiries respecting the improvement of poor soils.

We must never forget, that plants contain a living principle, that the action of this principle seems to be analogous to the power which fire has of altering the arrangement of the particles of matter; of elevating some into the form of *gas*, and of rejecting others; and that the final cause of life, in every individual, is to bring together such particles of matter, as, when duly acted upon and assimilated, will constitute the essence of each particular living being. Thus, from the same nourishment do different living powers produce totally distinct matters, only by new arrangements: and in his laboratory the chemist, from various and different proportions of the same ingredients, can constitute and produce results, more different, in their properties and appearances, than any two species of plants or animals.

All the alterations which the earths undergo when by heat they run into fusion, become fluid, or rise into vapour, are produced by operations very similar to those of digestion and chylification in the body. Every particle of matter, by one process or another, is capable of being converted into aeriform fluids, which, in rising from the surface, meet, intimately mix and form new compounds. The same may be affirmed of composts: the intermixture of various substances produces decomposition, particles, formerly united, are separated, and new arrangements take place.

* The nature of the lime employed must be attended to according to the nature of the soil to be improved.† Chalk, when burnt into Lime, contains from 5 to 10 per cent. of sand or clay, whereas some lime-stones contain from 50 to 80 per cent.; some also contain Magnesia, which according to Mr. Tenant, Philos. Trans. is not only not useful in agriculture, an improver of the soil; but hurtful to vegeetable life. Magnesian lime-stone may be discovered by the slowness with which it dissolves in acids; and it may be easily detected in chalk, by adding a sufficient quantity of the vitriolic acid, which, uniting with the magnesia, forms the bitter purging salt very distinguishable by its bitter taste.

† This opinion of Mr. T's, is controverted, by Mr. Headrick. See Farmer's Mag.

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All the products of nature seem destined to perpetual change and alteration; and the fibrous roots of plants appear intended by providence to produce the first stage in the transmutation of inert matter into life. Thus, by decomposition and absorption, earth becomes vegetable; vegetable matter is no sooner decomposed in the stomach of animals, than it is capable of being converted into animal matter; and when farther purified by the delicate organs of the human body, reaches the utmost perfection of created intelligence. *

Having thus generally stated the necessity of a mixture of earths, in the formation of a good soil, and pointed out the reason for that necessity, I shall beg leave to particularize a few things more in answer to the question.

It has been a commonly received opinion, that oil is the principal food of plants; but oil can no more enter the fine vessels of plants, than any one of the simple earths; it must therefore be decomposed and resolved into its elements, as well as any thing else. Oil may, and probably does, contain a very large portion of the substance which constitutes the chief food of plants in certain stages of their growth; but it must be decompounded to produce digestion, in the same manner as we have proposed in the admixture of the different earths. Alkalies and lime will render oil capable of mixing very intimately with water; and we are thence led to conclude, that they may contribute to render it more digestible, and thus capable of entering into the composition of the plants destined to be nurtured.

This doctrine may be farther illustrated by the process which milk undergoes in the stomach and bowels —Milk does not enter the lacteals of animals; but must undergo decomposition, and be digested, as well as any other food, before it can serve the purposes of nourishment.

There are however many other things to be done, before barren soils can be productive, and which may be be done where the due admixture of the earths is not to be obtained.

* " And the Lord God formed Man of the dust of the ground *Genesis* ii. 7."

" For thou exist'st on many a thousand grains
that issue out of dust." SHAKESPEARE.

There

There are various processes found adapted to particular soils, the introduction of which may reward the industry of the husbandman. Observations and enquiries respecting the improvement of poor soils.

1st. Thus the wolds of this country have been enriched by the cultivation of saint-foin, and tons of hay are now produced, where one blade of grass could scarcely have been found a few years ago.

2ndly, Thistles, which are capable of deriving nourishment, and growing to a large size, where no other plant can exist: these by the exuviæ, or remains they leave, and the protection they afford to other plants, and many animalculæ, tend to ameliorate the soil; but, whether they should be suffered to grow to a crop, and advantage taken of their product, or ploughed in as manure, is a question which I shall not agitate at present.

3rdly, The cultivation of spinach may be recommended as calculated to answer the same end, the prickly kind being the hardiest is to be preferred. Succulent plants impoverish the ground but little, because they derive a great part of their nourishment from the atmosphere, as may be easily proved from the aloe tribe, which will lie out of the ground for a great length of time without being hurt, drawing their nourishment from the atmosphere alone; and certainly these fleshy succulent plants, when ploughed in, will afford a very considerable supply of food for more useful plants*.

4thly, Buck-wheat also, and fumitory, a common weed upon chalky soils, may be converted to every useful purposes as a stimulus to vegetation; for the latter, when burnt, affords an uncommon quantity of the fixed alkali, so well known to be a most powerful stimulus to the growth of plants; and as the poorest soils may, by a particular management in the use of stimuli, be made productive, so an alternate crop of such plants with corn, seems to be an eligible mode of cultivating poor soils, where lime and manure are not to be had †.

* " All succulent plants make ground fine and of a good quality." *Vide Biberg' Œconomy of Nature.*

† In the 3d Volume of the American Transactions, there is a paper on the cultivation of the eastern shore Bean, for the express purpose of being ploughed in as a manure.

5thly.

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5thly, The planting of forest trees, as tending to defend the more valuable plants from the injury they are exposed to in a poor soil, is an object well worth attention; more particularly on grass land. Some author, in the Academy of Sciences, has proved, that land exposed to a long current of wind, which blew over a large tract of barren waste, would produce nothing but poor grasses, so long as it remained thus exposed; but, when this current was broken by a few hedges and plantations of forest trees, it became capable of propagating and rearing the most useful and prolific plants. Perhaps the atmosphere attracted by the trees, parts with its electrical matter, which has been found highly conducive to the growth of plants. The agitation given to the air, when driven against the hedges and trees, may dispose it to a decomposition highly favourable to its yielding nourishment; and on this principle, I apprehend, that in districts where the air is partially obstructed by hedges and trees, it always tends more to the amelioration of land, than where stone walls and mud fence are employed.

6thly, Planting oziers, on wet land, is another mode of answering the end proposed in the question. Lands not worth half-a-crown an acre on the side of the Trent, have been planted with oziers, at the expence of four pounds per acre, and since let for four guineas an acre per annum.

7thly, One source of barrenness in soils is the presence of the calx of iron. The calx or rust of iron may be known by the redness or blueness it gives to most soils, with which it is incorporated. It may appear extraordinary to many, that this iron should be the result of vegetation, but the fact is incontrovertible*. I have reason also to believe, from observation, that some trees and plants, are more disposed than others to produce the mineral earth; and it behoves the improver of the soil to ascertain, what these plants and trees are. Of trees, the willow tribe, and alder; amongst plants, the whole order of rushes, and above all, mosses, most assuredly abound in iron, and ought never to be suffered to exist on cultivated land.

* Vide Thompson's Chemistry, Vol. IV. page 228; Chaptal. Vol. III. page 170.

8thly,

8thly, The action of water upon soils in general ought not to be overlooked. Lying long upon the ground, it certainly tends to the destruction of those plants we wish to cultivate. Observations and enquiries respecting the improvement of bad soil.

Hence in all countries contrivances should be resorted to, to carry off the water, when its continuance would produce this effect.

But thousands of acres are barren for want of water; and there are few situations in which, if kept up in reservoirs, it may not be employed at times, with considerable advantage*.

In a variable climate, and a cultivated country, like ours, all the water that falls might be employed in agriculture. In the present state of things, perhaps, the expence might be greater than the profit; but, should engine work be so far improved, as to reduce the price of labour, and be introduced into practical husbandry, it will then, in a level country like Holderness, be no very difficult matter to place reservoirs and drains in such a manner, that a whole farm may be either drained or flooded at pleasure.

The Chinese, who certainly possess the best cultivated country in the world, are not content to make canals for the purposes of trade, they dig many others to catch the rain, with which they water their fields in time of drought; during the whole summer, the country people are busied in raising this water into ditches, which are made across the fields; in other places they contrive large reservoirs, made of turf, whose bottom is raised above the level of the ground about it; and, if they meet with a spring of water, it is worth while to observe how carefully they husband it; they sustain it by banks in the highest places, they turn it a hundred different ways, they divide it by drawing it by degrees, according as every one has occasion for it, in so much that a small rivulet, well managed, sometimes carries fertility to a whole province †.

* With a double view, *catch-water drains*, as they are called, ought to be formed, not only to prevent the low lands being flooded during violent rains; but, by keeping up the water, to preserve it to be employed, at a proper season, in irrigation.

† Vide Le Compt's Letters on China.

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Considerable expence has been incurred in this country, in order to find the best means of carrying off the water; but sufficient attention has no where been paid to the improvement of the soil, by the introduction of water for irrigation *.

Great advantages, of late years, have been derived from *warping*, along the banks of the Ouse, the Trent, and the Dutch river, where the water is let in at the flood tide, and suffered to rest, and deposit its mud, until the ebb. By this process, repeated twice a day during five or six summers, a new soil is formed, to the height of six feet, which, in the following spring will be firm enough to receive seeds, and in summer to carry an ox. Thus land, which was before only a peat bog, comparatively worth nothing, may be let for forty-five shillings per acre.

The Dutch river affords the best warp, because it nearly empties its whole channel during the ebb, and consequently contributes only the tide, there being very little back water during the flooding season; and hence, too, dry summers are better than wet ones; for, when the freshes are out, the water, though muddy, contains nothing but clay, washed from the tops of mountains, and the banks of rivers; but the muddy water of the tide contains all the products of the Humber, which consist of a large quantity of animal matter, as well as various species of earths.

An enterprising and spirited individual has proposed to warp the whole of several parishes extending over many thousand acres of bog, for one sixth of the land gained; which he purposes to effect by cutting a general canal, through these parishes from the Dutch river into the Trent †.

9thly,

* The whole of the low lands of Anlaby and Hessle might have been watered at pleasure, by keeping up the spring that passes through Anlaby town; or, by boring and piping the springs that may any where be found, and which will rise, in most places within those parishes, above the surface.

† The farmer, the land-owner, and the public, have all been benefited by this improvement. The farmer, by his industry and attention, has converted the most barren bog into land capable of bearing the plough, and of feeding an ox. The landlord by only foregoing the rent during the time the land is under water, has been able in a few years to increase the value of his property fifty fold.

The

9thly, Those whose lands border on the Humber, or the sea, may derive a further advantage from this vicinity, than what arises from mere irrigation. I have already taken some pains to point out the absolute necessity there is for bringing the different earths into close union, in order to procure that decomposition, necessary to their being converted into vegetable life; the same doctrine is applicable to composts, and may now be extended to salt water.

Observations and enquiries respecting the improvement of poor soils.

Salt water consists of certain alkaline salts united to the marine acid, which form a neutral, not easily decompounded in common earth, and therefore not a very active manure. To obtain the greatest possible advantage from sea water, it ought to be decomposed, which may, in part be effected by adding to it gypsum, alabaster, or plaster of Paris—a matter compounded of lime and the vitriolic acid. When this is well soaked in sea water, the vitriolic acid will in time quit the lime, seize the alkaline basis of sea-salt, and leave the marine acid to combine with the lime; * but in all these operations a large quantity of earth or soil should be compounded with the result, before it be applied as a manure, the salts being of themselves too pungent, if applied to vegetation unmixed with earth. This method ought also to be pursued, when any composts are formed.

10thly, Sand is also capable of further use than what is merely pointed out by the foregoing theory. In Norfolk it is thrown into the yards and stables, to absorb all the moisture; and the horses and cattle that are fed in the stalls, with cut grass or vetches, are bedded with it, in order that their urine may be absorbed and employed for the future amelioration of the soil.

11thly, The banks along the old or natural drains, which

The country at large not only obtains an increase in the supply of the food of man; but thousands of acres of the most noxious fens, prolific only in agues and remittent fevers, are by this process, covered with a healthful appearance.

* According to Berthollet, chalk is capable of decomposing sea salt, in the course of four years, and by that process, the natron of alkali is suffered to chrystalize in the lakes in Egypt *.

* Vide Memoirs on Egypt.

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have been formed by the overflowing of particular tides, when the Humber was not so well restrained within its limits as at present, are capable of being employed, to improve the soil. It is well known, that where the water first begins to deposit, there the best soil is produced; and, as these banks have been formed by repeated depositions of this kind, they consist, several feet deep, of valuable earth, which may be led away, and employed as a manure.

APPENDIX.

IN answer to those gentlemen in this Society, Mr. President, who have said, there is no land in Holderness bad enough to grow thistles upon, I ask, is there no land that requires occasional fallowing? If this be allowed, then the question will be, whether the cultivation of thistles be, or be not, more advantageous to the land, or productive to the farmer, than letting it lie fallow. Now it having been stated by such authority as Dr. Withering, that * thistles grow and flourish upon clays, where no other plant can exist without manure, and that, where they have grown, other plants may afterwards be propagated, will not a crop of thistles be found highly advantageous to the farmer? + For if they exist upon land, and draw none

" * Thistles, as the most useful, are armed and guarded by nature herself. Suppose there was a heap of clay, on which, for many years no plant had ever sprung up, let the seeds of the thistles blow there, and grow, the thistles, by their leaves, attract the moisture out of the air, send it into the clay by means of their roots, will thrive themselves, and afford a shade. Let now other plants come, and they will soon cover the ground." Biberg's Œconomy of Nature, translated by Stillingfleet. See also Withering's Botanical Arrangement.

+ " It is probable," says Parkinson, " that the sow-thistle, were it properly cultivated, would become one of the most fattening plants the earth produces. Sheep, when in clovers, &c. will feed upon it so greedily, as to eat the very roots; pigs likewise prefer it to almost any other green food; rabbits will breed more speedily when fed with sow thistles, than with any other food I know

none of the common nourishment from it, will not they, in a state of decomposition, be a valuable addition to nourishment, or at least prove a powerful stimulus to more valuable plants, which we may afterwards wish to cultivate upon the same land. Observations and enquiries respecting the improvement of poor soils.

'It is not the chief end of the existence of plants, to bring dead earthy matter into a state of life? We know, that when there is a due mixture of the earths, any plant we wish to cultivate will thrive and produce this effect; and that, if we add a sufficient stimulus, or manure, then such plants will yield the largest increase; or even where there is not a due mixture, provided we can supply a large and repeated quantity of the stimulus that even there, they may for a season, be induced to make vigorous shoots and even perfect themselves. But in the case of barren soils, where this due mixture is not present, and where (as the question implies) the stimulus is not to be had, it is the object of our enquiries to find out a plant that will grow, and either yield an immediate profit, or, by improving the soil, enable others more valuable to succeed in future. Now, as the soil immediately referred to is confessedly clay, and as thistles will grow on it, and leave behind them such a quantity of refuse as will enable other plants to succeed, ought we not to recommend the cultivation of them on poor land, with the expectation that they will add more to the soil than they take from it, and so become improvers.

It.

know of, except dandelion, which is of the same nature, and is now sold in Covent garden market, to the breeders of tame rabbits. A man of my acquaintance, who was allowed better skill with stallions, than the generality of mankind, used to search for sow-thistles to give to his horse. We have a well-known and decisive proof of the nutricious properties of sow-thistles, in the fat wether sheep, fed to an amazing size by Mr. Trimnel, of Bicker Fen, near Boston, Lincolnshire, upon the fen land. This Sheep was bred by Mr. Hutchinson, in Hail fen, from a ram bred by Mr. Robinson, of Kirby, near Sleaford. He never ate any corn, oil cakes, &c. but fed wholly upon grass and herbage; being turned with many other sheep into a field of clover, this sheep was at first observed to search for sow-thistles, and would eat no other food whilst any of them could be found in the part of the field

Observations and enquiries respecting the improvement of poor soils.

It is an old farming maxim, that plants of the same species will not thrive successively on the same land, for, where one plant has died, another of the same species cannot live. This is the case with animal life, and with combustion, or fire; two processes extremely analogous to vegetation *.

Where a candle becomes extinct (provided no fresh air be admitted) another cannot be lighted, and where an animal has died, another of the same species cannot exist; but other combustible matters may be made to burn, and animals of a different species may, for a time, be made to live. Thus I would infer, that, where a crop of wheat has grown, been brought to perfection, and died, there another crop of the same kind will not succeed; although different kind of corn, pulse, or grass may.

Now the reason why the former phenomena take place, is partly by the abstraction or taking away of something from the air, necessary to the life or support of the first animal or combustible substance, and partly, by the giving out something, which, (though inimical to the individual that parts with it) is nevertheless, (so far from being hurtful to others) the very matter some kinds prefer to live upon. The same or similar processes may go on with plants; where a crop of wheat has grown, the materials for the sustenance of wheat may not only be absorbed, but the situation in which it has lived and died, may be so impregnated with its excretions †, as to be inimical to the life

field, that was hurdled off successively a little at a time; when the field was bare of thistles, his attendants gave him three times a day, from two to five pounds at a meal. This sheep, when killed, measured five feet from the nose to the tail, the rump or cushion eight and a half in depth, plate or fore flank of the same thickness, breast end seven inches, one yard five inches and a half round the collar, and weighed sixty seven pounds a quarter, avoirdupoise weight; the legs were estimated at forty pounds each, but if cut haunch of venison fashion, would have weighed fifty pounds each; for which the proprietor, Mr. Lumby, was offered *two shillings* a pound; so that the legs only would have brought ten pounds. *Vide Parkinson's Experienced Farmer.*

* Vide preceding Observations.

† This theory may be exemplified by a fact, which I have fre-

life of any future plant of the same species, although, as we said before, not for any other kind, until, by a proper succession, this very matter be attracted and absorbed into the substance of other plants; and thus we are enabled to point out the obvious principles that govern those rotations, which the experience of all ages teaches.

Observations and enquiries respecting the improvement of poor soils.

The great desideratum, or object of our enquiries, then will be, what are the best means of bringing together a fresh set of materials remaining in the soil? And, what is the succession best calculated to remove from the land, the dregs of former crops; or, what plants will best live and thrive where others have previously been cultivated? I know it is the general opinion of men of experience, in this part of the country, that fallowing can alone effect the former, and it is the general practice to make the black and white corn succeed each other, in order to effect the latter. Let us, however, enquire a little further. I am aware, that the fact (to account for which, I have ventured to frame a theory) has been denied by authority*, long celebrated in agriculture; I mean, "that wheat cannot be made to grow upon the same land, for two or three years successively;" and we are referred to an experiment made in a field belonging to Mr. Barlow, near York, for the proof of the contrary; but what does the experiment say? It says, that "plants of wheat were taken from a situation, in which they had stood the winter, and transplanted into a field that had grown potatoes; had been afterwards ploughed, harrowed, and rolled, and were pricked down an inch deep, and nine inches from each other;" and, "that it is proposed to do the same for several successive years, in order to determine the doubtful point, whether wheat can be raised for a number of successive years, upon the same land;" and, "that, instead of letting the land lie waste, under a summer fallow, it may be made to produce a crop of cabbages, turnips, pease, beans, potatoes, or summer vetches, as preparatory to its being planted with wheat.—Can this experiment militate in the

frequently observed; the fibrous roots of thorn, and many other trees and plants, where they enter chalk or clay, leave behind them an ochre, or irony mark.

* Vide Dr. Hunter's Circular Letter.

Observations and enquiries respecting the improvement of poor soils.

the least against the doctrine here advanced? or does not rather go to prove the truth of it: for, is it not clear, that, in order to succeed, it is necessary by transplantation, to remove the plants from a soil, out of which they have already extracted a certain portion of its nutricious matter, and in which they have already deposited something which might be hurtful, when they came to flower and seed? *

That the plan of transplanting wheat will answer I have not a doubt; it has long been practised by several gentlemen in Norfolk; and upon the principles here laid down and agreed upon, we can judge how it may prevent the necessity of fallowing, as it goes to prove what I have before hinted at in my theory, that, by sowing a summer crop of leguminous plants, such as pease, beans, vetches, &c. or the useful roots, turnips and potatoes, every thing hurtful to the growth of wheat may be taken from the ground. This, however, will be perhaps as profitably done by substituting a succession of other white corn, instead of wheat, in a regular manner.

From the foregoing, then we are led to conclude, that, by attention to a proper mixture of the earths, in order to bring various particles into intimate union, by frequent new combinations, and by a succession of plants dissimilar in their habits from each other, we may so far improve agriculture, as to have yearly crops from such a soil as ours; and that it will be possible, in time, to bring every acre of ground in this district into an almost equal degree of value †.

* Some warp land on the other side of the Humber produced wheat for seven years, successively, without manure; but, this only proves the possibility, that earth may be accidentally so well arranged and mixed, as to afford nourishment for a long succession of crops.

† There is a practice, frequent in Holderness, which deserves to be reprobated, and that is, suffering stubbles to lie unploughed after harvest. It appears to me a shocking waste of the valuable soil, to suffer it to be exhausted at the latter spring, in producing useless plants and weeds. The great object of agriculture, is to take advantage of every circumstance, that can oblige the earth to produce only the profitable parts of the vegetable creation; to suffer the land, therefore, to support what at present we know not the use of, is in the highest degree injurious and impolitic.

VII.

VII.

Description, of an Apparatus for transferring Gasses over Water or Mercury, &c. *By the* Rev. Gilbert Austin, M. R. I. A.*

THE difficulty of transferring gasses from one jar or receiver to another without loss, or mixture of atmospheric-air, by the common mode in the pneumatic apparatus, must have been experienced often by philosophical chemists. And this difficulty is encreased when very large jars are used, and when the production of gas in them is inconsiderable; as when oxygen gas is obtained from vegetables exposed to light, or from the decomposition of water. Of the small quantity, obtained in this manner, a portion is often lost in transferring it into a smaller jar for the purpose of subjecting it to examination; and the result of the experiment is rendered uncertain, if the object be to measure the quantity. In order to obviate this inconvenience, I beg leave to submit to the Royal Irish Academy the description of a small apparatus, which I have found to answer well, and conceive may be admitted as a useful instrument into a philosophical laboratory.

Difficulties of transferring gasses by the usual apparatus.

The principal part of this apparatus consists of two pieces of plate glass, with a hole of about half an inch diameter drilled through each. They should be something broader, and about twice as long as the diameter of the jars used in collecting and transferring the gasses. The holes should be disposed as in the figure. That in the plate (Fig. 1.), marked *(a)* should be nearly in the middle of the piece. The hole in the upper plate *(b)* near the extreme edge. The upper plate is shorter than the under plate, and its edge is grounded fair and straight, so as to fit the edge of the *third* plate, which is not drilled, and should be a square piece cut off the second plate, as it is very necessary that these two plates should be of the same thickness. The

A new apparatus in which these glass plates are applied to the mouth of the jar. &c.

* From the Irish Memoirs, 1806.

length

length of these plates together should exceed that of the under plate about an inch. It is rather better to grind the polish off the plates with a little fine emery, as they slide more equably over each other when so prepared. All the jars to be used with them should have their mouths ground on a flat plate with fine emery. Things being thus prepared, the transferring plates may be used in the following manner, particularly when the jars for collecting the gasses are large.

Improved apparatus for transferring the gasses.

When the jars, inverted in the usual manner in the pneumatic trough, are filled with the gas in any proportion, the two plates *(a* and *b)* are laid over each other in such a situation, that their holes shall not coincide; they are then plunged into the water, and the plate *(q)* applied to the mouth of the jar, and that and the plate *(b)* being moderately pressed against the mouth, so that they shall not slip, or suffer any gas to escape, the jar together with the plates, is lifted out of the water, and set with the mouth turned up. In this position the jar is ready for yielding the gas to the jar into which it is to be transferred. This last jar is now to be filled with water, taking care not to leave any air in it, and its mouth is to be closed by the third plate. It is then to be turned with its mouth downwards, and, together with the third plate on which it stands, is to be placed on that part of the under plate which is not covered by the upper plate. The edges of the third and upper plate are placed as nearly as possible in contact; and across them the small jar, filled with water, is to be slided till it rests entirely on the upper plate. The hole in the upper plate is to be filled with a few drops of water, and the jar is to be slided so as to stand over it. The upper plate, and the jar standing upon it, are then to be so moved over the under plate, that the holes in each shall coincide. The water in the upper jar, as soon as the communication is thus opened, will descend into the lower or magazine jar, and be supplied with an equal bulk of gas from below at pleasure. When a sufficient quantity is transferred thus into the upper jar, it is pushed together with its plate, in such a manner that the holes shall no longer coincide, and, consequently, the communication shall be cut off. The upper jar is slided back upon the third plate, and,

and, together with the plate, is removed in the same manner as it was applied. The mouth of the jar is turned upwards, the plate removed, and the gas submitted to examination: or, with mouth downwards, the small jar is placed on the shelf of the pneumatic trough, as the experiment may require. This detail appears tedious, but the practice is very easy. In this process there is, however, some danger of disturbing the lower plate, by lifting it from the mouth of the magazine jar, and so vitiating the gas by the introduction of common air. To prevent this inconvenience, it is necessary to secure the two perforated plates to the mouth of the jar, and to each other, allowing the upper plate, at the same time, to slide freely over the other. For this purpose, it is necessary to fix the plates, and the magazine jar, in a frame; which renders the use of them very convenient, and not liable to accidental disturbance.

Improved apparatus for transferring the gasses.

The two plates (*a* and *b*), as in Fig. 1, are fixed in the upper part of the frame: (*a*) is fastened (*b*) slides easily over it. The jar (*d*) is pressed up against the plate (*a*) by a moveable bottom (*k*), tightened by wedges or screws. The jar may be filled with water before it is fixed in the frame, and inverted in the trough; or the air may be generated in the jar, without the frame, and then, the frame being inverted; and the plates sunk in the water, the jar may be slipped into its place, and fixed there, which is the better way. The frame and jar are then set upright, and the gas may be transferred as before, without danger or loss, or mixture.

By means of this apparatus, jars of any size may be used as magazines, with the inconvenience of being obliged to invert them in large troughs.

This apparatus, also, on a smaller scale, may be used in operating with those gasses which can only be confined over mercury. The joints of the transferring plates retain very securely any quantity of mercury, provided the height of the jar is inconsiderable, not more than three or four inches, for reasons well known to experimental philosophers. And small jars, with ground mouths, hold mercury very well, when standing, without agitation, with their mouths downwards, on ground plates of glass. The careful operator will, however,

Improved apparatus for transferring the gases.

however, gently press them to prevent accidents. This apparatus may be so far reduced in size, that, on a small scale, all operations, on gasses only to be confined over mercury, may be performed with about four or five pounds of mercury: which may, in many cases, be an object of attention to the philosophical chemist.

Fig. 1. *(a)* The under plate; the dotted line marks the circumference of the mouth of the magazine jar.
(b) The upper plate.
(c) The third plate; the dots mark the circumference of the mouth of the small jar. The small dark circle shews the place of the holes.

Fig. 1. *(a b e)* The section of the plates, (as in Fig. 1.)
(d) The magazine jar.
(e) The small jar.
(f) The dotted jar shews how the small jar is placed, together with the third plate *(e)*, before it is slided across the edges *(g)* of that and the upper plate.

Fig. 3. *(a b c)* The plates as before, but fixed in.
(b) The frame.
(d) The lower or magazine jar, (as wedged up) against the under plate, by
(k) The moveable bottom.
(e) The small jar to be filled with gas from the lower jar.

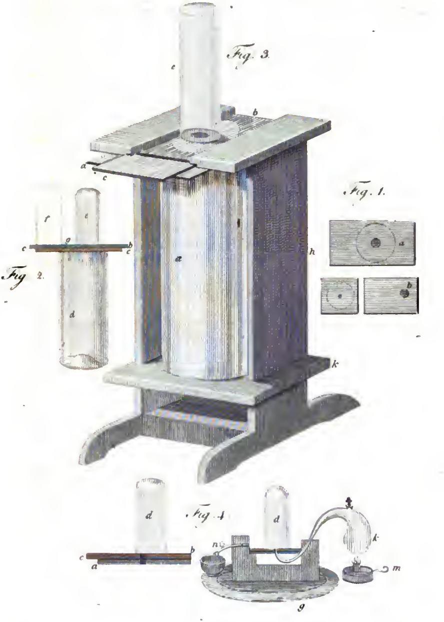
Fig. 4. *A small Apparatus for operating with Mercury.*

(a b c) The plates as before.
(d) The small jar, four inches high, with a broad rim, by which the lower plate may be confined to its mouth, together with a frame in which the upper and third plates may slide. This frame may be made of hard wood, of ivory, or of iron.

(g) A section of a wooden box, to hold as much mercury as will cover the plates and frame, and admit the bent tube of

(*l*)

Mr. Austin's Pneumatic Apparatus.



(*l*) A small retort or vial, with a bent tube, for generating the gas which passes through the hole of the plates. Improved apparatus for transferring the gases.

(*m*) A small spirit lamp.

(*n*) A tube, fixed so in the box, that the mercury, descending from (*d*) as the gas is generated, shall overflow, and be received in a cup; with which small jars may be filled for transferring.

VIII.

Description of the Mineral Bason in the Counties of Monmouth, Glamorgan, Brecon, Carmarthen, and Pembroke. By Mr. EDWARD MARTIN.*

THE irregular oval line, delineated on the annexed map † shows nearly the inner edge of a limestone bason, in which all the strata of coal and iron ore (commonly called Iron Stone) in South Wales are deposited; the length of this bason is upwards of 100 miles, and the average breadth in the counties of Monmouth, Glamorgan, Carmarthen, and part of Brecon, is from 18 to 20 miles, and in Pembrokeshire only from 3 to 5 miles. Mineral bason in South Wales.

2. On the north side of a line, that may be drawn in an east and west direction, ranging nearly through the middle of this bason, all the strata rise gradually northward; and on

* Phil. Trans. 1806.

† The outline on the map given in the Transactions (but not copied in our Journal) begins from the N. E. corner of St. Bride's Bay, and proceeds by Haverford West, across a small part of Carmarthen Bay whence it passes near Kidwelly more northerly till about three miles south of Llandillo. From this part it inclines more southerly towards Abergavenny, but within five miles of that town it rounds to the south through Pontypool and thence to the S. W. (rounding) through Llantrissent, but whence it arrives at the coast of Swansea Bay it spreads nearly in a line to Tenby, and thence to the middle of the Western shore of St. Bride's Bay.

2 the

Mineral bason in South Wales. the south side of this line they rise southward, till they come to the surface, except at the east end, which is in the vicinity of Pontypool, where they rise eastward.

3. The depths from the surface to the various strata of coal and iron ore depend upon their respective local situations.

4. The deepest part of the bason is between Neath, in Glamorganshire, and Llanelly, in Carmarthenshire; the uppermost stratum of coal here does not extend a mile in a north and south direction, and not many miles in an east and west direction, and its utmost depth is not above 50 or 60 fathoms.

5. The next stratum of coal, and those likewise beneath it, lie deeper and expand still longer and wider, and the lowest which are attended by parallel strata of iron ore, of which they are in some situations about 16 accompanied by irregular balls or lumps of iron ore, occupy the whole space between Llanmaddock Hill, near the the entrance of Burry river, to Llanbidie, from the Mumbles to Cribbath, from Newton Down to Penderryn, from Castle Coch to Castle Morlais, and from Risca to Llangattock, and in length of the south side of the bason from Pontypool and through Risca, Tinkwood, Llantrissent. Margam, Swansea Bay, and Cline Wood, to Llanmaddock Hill, and on the north side through Blaenafon, Ebbw, Sirhowy, Merthyr, Aberdare, Aberpergwm, Glyntowy, Llandibie, and the Great Mountain, to Pembrey Hill, near Llanelly in Carmarthenshire, and their depths are at the center range of strata from 6 to 700 fathoms.

6. The strata of coal and iron ore running from Pembry Hill, through Carmarthen Bay and Pembrokeshire, to St. Bride's Bay, are only a continuation of those in the counties of Glamorgan and Carmarthen, which lie next to and parallel with the north side of the bason, all the remaining strata rising southward; and the middle ranges on the north side of the bason, are lost between where they meet the sea near Llanmaddock Hill and the south side of Pembrey Hill, in their course towards Pembrokeshire, in consequence of a contraction of the sides of the mineral bason, or rather by its becoming shallower; for in Pembrokeshire none of the strata of coal or iron ore lie above 80 or 100 fathoms deep, consequently all those which do not lie above 5 or 600 fathoms in

in Glamorganshire and Carmarthenshire have not reached this county, by reason of the bason not being of sufficient depth and width to hold them. Mineral bason in South Wales

7. The strata of coal at the east end of the bason running from Pontypool to Blaenafon and Clydach and on the north-side from thence to Nanty Glo, Ebbw, Beaufort, Sirhowy, Tredegar, Remney, Dowlais, Penderryn, Plymouth, Cyfarthfa, Abernant, Aberdare and Hurwain Furnaces and Iron Works, are of a cokeing quality, and from thence the whole strata of coal to St. Bride's Bay alter in their quality, to what is called Stone Coal, (the large of which has hitherto been used for the purposes of drying malt and hops, and the small, which is called Culm, for burning of limestone); the several strata of coal from Pontypool. on the south side of the bason, through Risca, Llantrissent, Margam, and Cline Wood, to Burry River, Llanelly, and the south side of Pembrey Hill, are principally of a bituminous or binding quality.

8. Notwithstanding the principal strata of coal in Glamorganshire, lie from 5 fathoms to 6 or 700 fathoms deep, still it has not been necessary to pursue these strata deeper than about 80 fathoms.

9. The veins of coal and iron ore, in the vicinity of most of the iron works in Monmouthshire and Glamorganshire are drained and worked by levels or horizontal drifts, which opportunity is given by the deep valleys which generally run in a north and south direction, intersecting the range of coal and iron ore, which run in an east and west direction, under the high mountains, and thereby serving as main drains, so that the collier or miner here gets at the treasures of the earth, without going to the expence and labour of sinking deep pits, and erecting powerful fire-engines. However, in process of time, in situations where the coal and iron ore that are above the level of these natural drains, become exhausted, it will be found necessary to sink shallow pits, and erect fire-engines for the draining and working of the coal and iron ore, and at a future period, pits of greater depths must be sunk for the same purposes.

10. There are 12 veins or strata of coal in this mineral depository, from 3 feet to 9 feet thick each. which together make 70½ feet: and there are 11 more, from 18 inches to 3

Mineral Bason in South Wales. feet, which make $24\frac{1}{2}$ feet, making in all 95 feet; besides a number of smaller veins from 12 to 18 inches, and from 6 to 12 inches in thickness, not calculated upon.

11. By taking the average length and breadth of the foregoing different strata of coal, the amount is about 1000 square miles, containing 95 feet of coal in 23 distinct strata, which will produce in the common way of working 100,000 tons *per* acre, or 64,000,000 tons *per* square mile.

12. If the whole extent of this mineral country was an even plain, the border or outbreak of each stratum would appear regular and true; but owing to the interposition of hills, valleys, the edges of the strata, if nicely measured and planned, would seem indented and uneven, yet in many instances the due range is totally thrown out of course, in consequence of knots, dikes, or faults.

13. These faults or irregularities are not confined to the edges of the strata, but they take grand ranges, through the interior of the bason, generally in a north and south direction, and often throw the whole of the strata, for hundreds of acres together, 40, 60, 80, or 100 fathoms, up or down, and still there is seldom any superficial appearance, that indicates a disjunction, for the largest faults frequently lie under even surfaces.

14. As every stratum rises regularly from its base to the surface, and frequently visible and bare, in precipices and deep dingles, and often discovered where the earth or soil is shallow in trenching, or in forming high roads, and by reason of the whole of the country within this boundary being so perforated by pits, and so intersected by the various operations of art and nature, it is not probable that any vein of coal, iron ore, or other stratum remains undiscovered in this mineral bason.

15. Glamorganshire engrosses far the greatest portion of coal and iron ore, Monmouthshire the next in point of quantity, Carmarthenshire the next, Pembrokeshire the next, and Brecknockshire possesses the least.

16. The strata of coal and iron ore in the last named county, which are the lowest in the bason, break out northward, and only take place in the three following distinct spots, *viz.* 1st. From Turch River (which is the boundary between Lord CAWDOR and CHARLES MORGAN Esq.) across the river Tawe and the

the Drin Mountain to the great forest of Brecon. 2d. A corner of ground from Blaen Romney to the north of Brynoer. 3d. Another spot, from Rhyd Ebbw and Beaufort Iron Works, through Llwyn y Pwll, near Tavern Maed Snr, to where it joins Lord Abergavenny's mineral property. Mineral bason in South Wales.

17. *Note.* A principal fault is observable at Cribbath where the beds or strata of the limestone stand erect: another, of considerable magnitude, lies between Ystradvellte, and Penderryn, where all the strata on the north side of the bason are moved many hundred yards southward (as at Dinas.)

18. *Note.* The limestone appears to the surface all along the boundary line in the counties of Monmouth, Glamorgan, Carmarthen, Brecon, and no doubt can be entertained of its due range from Newton across Swansey Bay to the Mumbles, and from Llanmaddock Hill across Carmarthen Bay to Tenby. In Pembrokeshire it appears to the surface on the south side of the bason, at Tenby, Ivy Tower, Cockelard, Bit, Church-Williamston, Lawrinny, Cord, Canta, and Johnston, and on the north side of the bason, at Templeton, Picton, Harriston and Persfield; yet it certainly forms an underground connection from point to point.

IX.

On the Water Pits of the Glaciers of Chamouny. By a Correspondent.

To Mr. NICHOLSON,

SIR,

Cork, 15th April, 1807.

I JUST now was looking over a paper from Count Rumford, in your Journal, on a curious phenomenon on the "Glaciers of "Chamouny," with respect to a pit which he observes was formed in the ice. The manner in which he accounts for it, is, I think, inconsistent with his own rules. The manner in which I would account for this phenomenon, is, that as cold water Observations on a fact observed by Count Rumford.

 lying

lying on ice melts it sooner than warm water, so for the same reason the water which lay on the top of these pits was, as he observes, warmed by the wind which passed over them, and that which was in the bottom of the pit cold, of course it had a tendency to melt downwards rather than at the sides. Now, Sir, if you will have the goodness to publish this in your Journal, so that Count Rumford may see it, you will oblige me in hopes that I may see what he thinks of my account of this phenomenon.

I am,

Sir, &c. &c.

A constant Reader.

X.

Remark by M. De Lalande *on the Distance of the Stars.*

Distance of the stars.

DURING the last century it has been believed, that the annual parallax of the stars, that is the difference of their situations in the course of six months, relative to the position of the earth, does not vary a single second; whence it results that their distance must exceed seven millions of millions of leagues.

M. Piazzi, at Palermo, and M. Callandrelli, at Rome, have recently made observations on several of the stars, from which it appears that some of the stars give a difference of five seconds, particularly Lyra, which, next to Sirius, is the most brilliant star in our hemisphere, from whence it results that it is one of the least distant. If there be five seconds of simple parallax, the distance ought to be fourteen hundred thousand millions of leagues, that is to say, five times less than was previously supposed. But these observations are not yet sufficiently numerous and complete, to afford a perfectly certain conclusion.

XI.

XI.

Observations on the Soda, Magnesia, and Lime, contained in the Water of the Ocean; shewing that they operate advantageously there by neutralizing Acids, and among others the Septic Acid, and that Sea-Water may be rendered fit for washing Clothes without the Aid of Soap. By SAMUEL L. MITCHILL, *of New York.**

MANY attempts have been made to render the water of the ocean fit for the purposes of *drinking* and *cooking*, and some of these have been attended with flattering prospects of utility. By a cheap and easy process, water tolerably fresh may be distilled from common salt water, so as to help materially in a case of scarcity or want, on board a ship of good equipment. The names of Hales, Lind and Irvine, are remembered to their honour, for their exertions in this work.

Observations and facts respecting the component parts contained in sea water, and the useful applications of that fluid.

To furnish needy men with the means of *eating* and *drinking*, is certainly a noble discovery. But there is another operation scarcely less necessary to the preservation of health than eating and drinking, and that is *washing* as applied to the human body, and more particularly to the clothing which it befouls. In a communication to professor Duncan, which has been published in the Edinburgh Annals of Medicine for 1799, and in the third volume of the New York Medical Repository, I have endeavoured to state the facts in detail concerning the matters secreted from the skin and wiped off by the clothes, and to shew how some of these became unwholesome, or infectious and pestilential, as they grew nasty. It was there stated that soaps and *alkalies* would render foul clothing clean, and both prevent and destroy animal poison if it was engendering there. And in a letter I wrote to Timothy Pickering, late Secretary of State to the American Government, in November

* American Transactions, vol. v. The Doctor uses the term septic for azotic or nitric.

1799,

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1799, I recommended barilla or soda as a substance by which the salt-water of the ocean could be so softened and altered in its qualities as to become fit for washing the clothes of seamen.

A sea-vessel is peculiarly fitted for concentering foul and corrupting things, and for converting them into pestilence and poison. This is one of the most common accidents in sailing to the latitudes where there is heat enough to promote corruption and to exalt septic substances into vapour.

One of the most disgusting sights during a voyage is the personal nastiness of many of the crew. It is pretended that much of this is necessarily connected with the service, that the work is dirty, and especially that fresh water cannot be spared from the vessel's stores to wash the company's clothing; that soap cannot be used with ocean-water, that salt-water alone will not get them clean, and that therefore they are under a necessity of being uncomfortably nasty on long voyages, especially toward the latter part of them. Now, nastiness of a man's person and garments is necessarily connected with a similar condition of his bed, bedding, hammock and berth, and most commonly of every thing he handles or has ought to do with. If a seaman has strength of constitution to keep about and do duty, his feelings are nevertheless very uncomfortable, he is thereby predisposed to disease and in danger every moment of becoming sick; and if this should really happen, his chance of recovery is exceedingly lessened by the filth with which every thing that touches him is impregnated, and the venom into which that filth is incessantly changing.

Thus, the great difficulties with which a seaman has to struggle, are 1st, the unfitness of ocean-water to wash with; and 2d, the inutility of soap to aid that fluid in cleansing his clothes. If these can be surmounted, he will have no excuse for his uncleanness. If after this he becomes uncomfortable or sickly from that cause, it will be owing to his own laziness or negligence.

Few subjects have been discussed with more solicitude than the one, How did the ocean acquire its saltness? Whether that mass of waters derived its briny quality gradually by dissolving strata of salt, or whether it was furnished by its Creator with a due quantity of that material from the beginning, are

are questions not necessary now to be answered. It is sufficient to observe that it is kept sweet and guarded against offensiveness and corruption by the great quantity of *alkaline* matter it contains. The ocean may indeed be considered as containing some portion of every thing which water is capable of containing or dissolving, and its water is therefore found to furnish different results on analysis, when taken up from different depths and in different latitudes.

Observations and facts respecting the component parts contained in sea water, and the useful applications of that fluid.

Yet various as the composition of ocean-water is, it always contains *soda*, *magnesia* and *lime*, in quantity considerable enough to be easily detected. Of these *soda* is the most abundant. *Magnesia* is next in quantity. And *lime*, though plentiful, is believed to exist in smaller proportion than either.

The *alkaline matter* so plentifully dispersed through the water of the ocean, exerts its customary neutralizing power after the same manner and according to the same laws which govern its several kinds on the land and in other places.

The acids commonly present in ocean-water are the *sulphuric*, the *septic* and the *muriatic*. The former of these exists apparently in small quantity, and is only mentioned because in some experiments it has been said to have been obtained from it in the form of a sulphate of lime, though according to the law of attractions, we might expect to find in it sulphate of soda. The vast amount of animal matter existing in the sea, would lead one à priori to a persuasion that in certain cases, particularly along marshes and shores were the stagnating water was much heated, putrefaction would engender *septic* acid, and this would in some measure mingle with the water in its vicinity, and not fly away wholly in vapour. The quantity of this acid is so considerable in some coves and bays where salt works have been established, that a quantity of it adheres to the muriate of soda or common salt and vitiates its quality. And this happens in some situations to so high a degree, that Neumman (Chemical Works by Lewis, p. 392,) takes notice of it, observing " that sea water often contains a *nitrous* matter, the *acid spirit distilled from sea salt proving a menstruum for gold*, which the marine acid by itself never does, and which nothing but the

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the nitrous will enable it to do. Though however this is frequently the case, it is not always: I have examined marine salt whose acid had no action upon gold."—As to the *muriatic* acid, whether it is as some of the older chemists suppose a modification of the sulphuric and the nitrous, or as certain of the moderns believe, but a compound basis of sulphuric and hydrogene, there is evidence enough of its existence in the ocean in very great plenty.—On the whole, it may be concluded that sea-water *always* contains *muriatic* acid, *frequently septic* and *sometimes sulphuric*.

There are thus three predominating *alkalies* and as many *acids* in the ocean; and by the intervention of water they are liquefied and put in a condition to act each upon the other. Consequently the soda in the first place, as the stronger alkali attaches and neutralizes the acids in the order of chemical affinity, and forms sulphate, septate and muriate of soda. But as the *two* former are comparatively rare or scarce, the latter is the predominating compound. When there is any acid in the water beyond the capacity of the soda existing there to neutralize, that part is attracted by the *two* earths, and acording to the force of their respective combinations, forms sulphates, septates and muriates of lime and magnesia. These salts with earthy bases, in which the muriatic acid is by far more abundant than the other two acids, constitute the *bittern* and *scratch* or *slack* of the salt makers. These salted earths attract water so strongly that it is difficult or impossible to make them crystallize; but wherever they are they keep up a dampness and refuse to dry.

When chemists speak of sea salt they wish to be understood as meaning " the pure muriate of soda." This neutral compound however in its *pure* state is a great rarity. Perhaps indeed there is no such thing. Experience shews it is always mingled with greater or less quantities of the *deliquescent salts with earthy bases*. And these are so abundant in some sorts of salt that they render it unfit for the preservation of animal provisions. Beef and even pork, are not guarded by salt so adulterated, from becoming tainted and putrid. That sea salt of this impure quality should be fit for

curing

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curing provisions, it ought to undergo a particular refining operation to rid it of its foreign admixtures. For want of such a process, some sorts of sea salt, though fair to the eye, do not possess an intire and undivided antiseptic power, but so far as the muriate of soda in the mass is alloyed by the middle salts of magnesian and calcarious composition, those parcels of common salt so vitiated become unfit for opposing completely the process of putrefaction. And so far they make a departure from the antiseptic power of pure muriate of soda, the manner of whose action, I endeavoured to investigate in a Memoir addressed to professor Woodhouse and published in the second volume of the New York Medical Repository.

By reason of these foreign and adventitious matters, it happened in Sir John Pringle's experiments, that the common salt employed by him, instead of preventing the corruption of meat, when added in small quantity rather promoted its decay. (Paper III. Exp. 24.) His trials he observes were made with the white or *boiled* salt kept there (in London I suppose he means) for domestic uses. (Appendix to Observations on Diseases of the Army, &c. p. 345, Note.). This kind of salt is known to abound with the *earthy* salts with which ocean water is charged.

Dr. Percival's experiments on sea salt have a tendency to shew that the septic quality ascribed by the learned Baronet to small quantites of common salt is owing to the mixture of *bitter salt* with it. A quantity of this, he observes, adheres to all the common salt used for culinary and dietetic purposes, and as far as its influence goes, it counteracts the wholesome and preservative powers of the clean and unmixed muriate of soda (1 Essays Medical, &c. p. 344,) and that this *septic* quality of the sea salt depended upon the presence of some heterogeneous substance was the opinion of Pringle himself. (Ibid. p. 347.)

Such then being the composition of ocean water, it is easy to explain wherefore it is not fit *by itself*, for washing garments and making them clean. It has a deficiency of *alkaline salt* in it; and alkaline salts are well known to be the most excellent and complete detergents. And it is quite as

easy

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easy to assign a reason why it will not answer to employ *soap* with ocean water. The acids united to the lime and magnesia being more strongly attracted by the alkali of the soap, quit their connection with those earths, which fall to the bottom, while the lighter and deserted oil rises to the top. The activity of the alkali of the soap thus overcome by the neutralizing acid of the water, can be of little service, and the disengaged grease immediately thereafter becomes a real impediment.

The basis of all *hard* soap is *soda*. The alkaline matter of *soft* soap is *potash*. This probably happens because the former is prone to *effloresce*, the latter to *deliquesce* in the air. The reason of mingling oil, turpentine and tallow with potash is that this salt is too corrosive to be handled naked or alone. By its causticity *potash* destroys the skin and flesh of the washer, and unless carefully employed, will destroy the goods too. But this is not the case with soda; which in conjunction with carbonic acid may be dissolved in water without exercising any caustic effect upon the arms and fingers of the person who uses it. By virtue of this convenient and excellent quality, the carbonate of soda can not only be used in a lixivial form to cleanse goods, but may be employed to alkalize or soften ocean water and to render it fit for washing with.

It has been ascertained long ago by Professor Home in his experiments on bleaching, that neither sea salt nor any other of the *perfectly* neutral salts composed of an acid and an *alkali* give any *hardness* to water; that the common sorts of sea salt make water *hard* by means only of the heterogeneous salts they retain from the *bittern*; and that *alkalies* by precipitating the earth of salts with an earthy basis and by neutralizing their acids, will *soften* water.

Ocean water, it has been shewn, besides a *perfect* neutral salt, contains a quantity of saline matter with *earthy* bases. To these latter, it owes its *hardness*, or quality to decompound soap. Carbonate of soda decomposes these *terrene* salts and forms with their acids respectively *perfect* neutral salts. The water thereupon becomes *soft*, or in other words, fit for washing goods.

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